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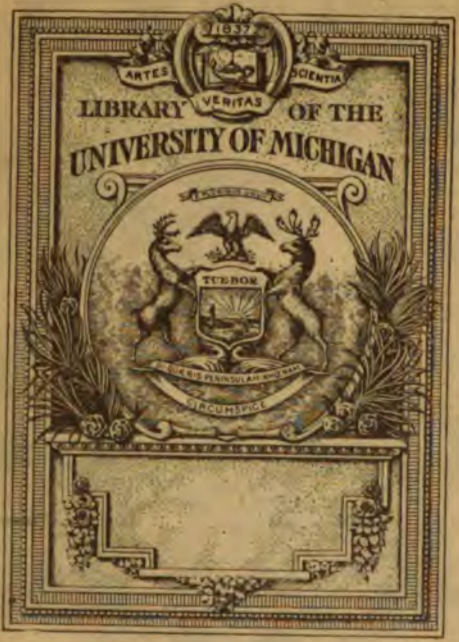

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ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS,

NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

BY THOMAS THOMSON, M.D. F.R.S. L. & E. F.L.S. &c.

MEMBER OF THE GEOLOGICAL SOCIETY, OF THE WERNERIAN SOCIETY, AND OF THE
IMPERIAL MEDICO-CHIRURGICAL ACADEMY OF PETERSBURGH.

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JANUARY TO JUNE, 1815.

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1815.

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NOTICE.

THIS volume will be found uncommonly rich in new and important scientific facts. It will not probably be often our lot to include so great a number in so small a compass. This is partly owing to the sudden opening of the Continent, which has enriched the historical article commencing the volume with a vast number of new facts, many of them of great importance. The two most prominent papers in a scientific point of view are, 1. The experiments by Berzelius on the composition of organized bodies. It brings a new kingdom under the dominion of chemistry; while the uncommon accuracy of Berzelius has given a degree of perfection to his labours that could hardly have been expected. 2. Gay-Lussac's paper on Iodine. It proves to be a very interesting body; and the new views which its properties lay open greatly increase the interest which the scientific chemist must take in these investigations.

May, 1815.

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ERRATA IN VOL. IV.

Page 420, line 42, *for* and portions of the gneiss, *read* not portions of the gneiss.
—— 421, —— 34, — a granite through gneiss, *read* or granite through gneiss.
—— 421, —— 36, — sea coast, *read* east coast.
—— 423, —— 25, — interrupted, *read* intermixed.
—— 448, —— 34, — pinnæ, *read* donax.

ERRATA IN VOL. V.

Page 56, line 12, *for* $+\sqrt{-1}$, *read* $\pm\sqrt{-1}$.
—— 56, —— 18, — $y = \sqrt{aa - xx}$, *read* $y = \pm\sqrt{aa - xx}$.
—— 58, —— 22, — experimental, *read* exponential.
—— 59, —— 14, — $\pm 2\pi\sqrt{-1}\left(\frac{26 \times 38}{4}\right)$, *read* $\pm 2\pi\sqrt{-1}\left(\frac{26 + 38}{4}\right)$
—— 59, —— 16, — $\pm 2\pi\sqrt{-1}\left(\frac{34 \times 34}{4}\right)$, *read* $\pm 2\pi\sqrt{-1}\left(\frac{34 + 34}{4}\right)$
—— 60, —— 25, — 185, *read* 55.

There is an error in the diagram, Plate XXVII., which, referring only to the line that is drawn through the centre, may be corrected without a new plate. The small o at the centre should be a large one, and the consecutive figures *under* the units and *above* them should refer to the circles which they touch. Thus $\underset{o}{1}$ is right, but the following is $\underset{o}{1}$, which should be $\underset{1}{1}$, and so on.

Page 218, morning observation, May, 1814, *for* 24° 12′ 49″, *read* 24° 13′ 12″; difference *for* + 0′ 47″, *read* 1′ 10″.
Page 218, noon observation, Nov. difference *for* — 0′ 13″, *read* + 0′ 13″.
—— 218, morning observation, Jan. 1815, *for* 24° 16′ 26″, *read* 24° 16′ 46″.
—— 229, line 4, *for* Macleugh, *read* Macleay.
—— 397, —— 1, — or Changelica, *read* Archangelica.

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ANNALS

OF

PHILOSOPHY.

JANUARY, 1815.

ARTICLE I.

Sketch of the latest Improvements in the Physical Sciences.

By Thomas Thomson, M.D. F.R.S.

AFTER an almost total exclusion from the Continent for about seven years, all the kingdoms of Europe have been suddenly thrown open; and it has been in our power, by importing the different foreign journals, to make ourselves acquainted with the various additions which the sciences have received during this eventful period. But these journals amount to so many volumes, that I find myself overwhelmed with matter, and foresee with regret that it will be utterly impossible for me to lay before my readers an historical sketch of the improvements which have been made in all the physical sciences. I shall therefore be under the necessity of confining myself, at least at present, to those sciences which have been cultivated with the greatest ardour, and in which the most important improvements have taken place. These are *Chemistry* and *Mineralogy*. I shall pass more slightly over what has been done in these sciences in Britain and France; because the journals and scientific works of these countries have already become in some measure known to my readers; partly by the contents of the *Annals of Philosophy*, and partly by means of the other London scientific journals. I shall dwell chiefly on what has been done in Germany and the North of Europe; because the languages of these countries are not much cultivated in Britain, and because our connection with them was so completely interrupted, that there is a considerable probability that most of the facts which I shall state are unknown to at least the greater number of my readers.

Next to chemistry and mineralogy, electricity, magnetism, and optics, have been studied on the Continent with the greatest attention. I shall take a subsequent opportunity of laying the improvements made in these sciences before the reader.

CHEMISTRY.

This science embraces so vast a field, and is cultivated by so numerous a tribe of votaries, that its annual progress is exceedingly striking and rapid. I shall, for the sake of perspicuity, arrange the facts I have to state under different heads; because I consider any arrangement, even though imperfect, as greatly preferable to no arrangement at all.

I. *General Principles.*

There are two general principles in chemistry of the utmost importance, which have lately attracted a great deal of the attention of chemists, and concerning which various theories have been started which have had considerable influence on the science. These two principles are, 1. The *power* by which bodies unite chemically. This power is usually known by the name of *affinity*. 2. The *proportions* in which bodies unite chemically. The facts established relative to this point have received the name of the *atomic theory*. I shall endeavour to lay a short view of the opinions relative to these two principles before my readers.

I. *Affinity.*—In the year 1803 an elaborate set of electrical experiments on the decomposition of salts and other bodies by the Galvanic pile was published by Hisinger and Berzelius.* This paper was republished in 1806 by the authors in the Swedish language.† In 1803 an abstract of this paper was translated from the German, and published by the French chemists at Paris.‡ Among other conclusions from their experiments, the authors draw the following: Substances are decomposed by electricity according to a determinate law. Oxygen and acids are attracted to the positive pole; while hydrogen, alkalies, earths, and metals, are attracted to the negative pole. This they consider as owing to an affinity subsisting between oxygen, and acids, and positive electricity; and between hydrogen, alkalies, earths and metals, and negative electricity. This important principle was still further developed by Sir Humphry Davy, and placed in a very luminous point of view in his celebrated lecture On some Chemical Agencies of Electricity, published in the Transactions of the Royal Society for 1808, and which gained the prize proposed by Bonaparte for the most important discovery in galvanism. This dissertation deserves, in every point of view, to be considered as the most valuable of all Sir Humphry Davy's philosophical discoveries. His subsequent discoveries were more brilliant, and gave him greater eclat; but they were all derived from this profound dissertation, which pointed out the means of employing galvanism as an instrument of analysis, and the probability by means of it of decomposing many bodies which had previously resisted all the efforts of chemical experiment.

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* Gehlen's neues allgemeines Journal der Chemie, i. 116.

† Afhandlingar i Fysik, Kemi och Mineralogi, i. 1.

‡ Annales de Chimie, li. 167.

In this dissertation the author showed that bodies which have a chemical affinity for each other are in different states of electricity; the one being negative, and the other positive. Thus when quicklime and oxalic acid are brought in contact, and separated, the acid is found to be negative, and the lime positive. Hence the reason why oxygen and acids are attracted by the negative pole of the battery. They are charged positively; and it is a law in electricity that bodies in different states are attracted by each other. Hydrogen, alkalies, earths, and oxides, are in a positive state of excitement; therefore they are attracted to the negative pole of the battery. Sir H. Davy conceives, with Volta, that there is a peculiar state of electricity, either positive or negative, which belongs to every body; that bodies which have an affinity for each other are in different states, and that the degree of affinity is proportional to the intensity of these different states: or, in other words, the more positive one body is, and the more negative another, the greater is the affinity between them. If we bring them into the same state, that is, render them both positive or both negative, they will no longer remain in combination, but will separate from each other. Hence the reason why common electricity and the galvanic battery decomposes bodies; and it is probable that by its means most bodies may be decomposed, because the affinity between all bodies must be definite, whereas the galvanic intensity may be increased at pleasure almost to any amount. According to this view of the subject, chemical affinity is the same power with the attraction which exists between bodies in different states of electrical excitement; and if any means can be fallen upon to measure this power, we shall at the same time have a measure of chemical affinity.

Such is an outline of the hypothesis of Sir Humphry Davy. Every one, I conceive, who has considered it with attention, must admit the great importance of it as a means of advancing our knowledge of the constituents of bodies, and be aware that it was in consequence of it that Sir H. Davy was led to attempt the decomposition of the alkalies and earths, and to obtain the splendid success with which these fortunate attempts were attended. He will recollect also the facts advanced in support of it by Volta and Mr. Brande, and the happy facility with which it enables us to explain many things that formerly appeared exceedingly obscure.

I am far from pretending to call in question the probability that this hypothesis may ultimately turn out accurate, and think it entitled to the closest attention of every chemist and electrician; but if it be correct, I conceive that the theory of electricity, which at present prevails, will not be able to stand its ground. If *negative* and *positive* electricity be qualities inherent in bodies, and continuing in them after they unite together, I cannot for my part conceive the one to consist in a *deficiency* of electric matter, and the other in an *excess* of it. Neither can I conceive with M. Dufay, the Abbe Haüy, and some other French electricians, that negative electricity consists in *one fluid*, and positive electricity in *another*, which have an attraction for each other, and neutralize each other when they come in contact. But I can conceive negative and positive electricity to be two attractions inherent in different bodies, which make them unite with each other, and keep them united. But if we adopt this view of the subject, we are not only embarrassed by the difficulty of explaining many electrical phenomena, but we lose the benefit of all the facts stated by Volta, and of most of those advanced by Sir H. Davy in favour of his hypothesis. I myself, therefore, though disposed to think favourably of the Davian hypothesis of chemical affinity, consider it as very far from being sufficiently established to constitute the basis of our researches and our arrangements.

This, however, has been done by Berzelius, who has bestowed much attention on it, and is certainly far better acquainted with the facts brought to light in support of it than I can pretend to be. His view of the subject does not differ much from that of Sir H. Davy; but the additions which he has made when we come to compare them with the present theory of electricity, or with any theory hitherto proposed, are sufficiently puzzling. According to him the acid or alkaline nature of a body depends upon the state of its electricity. If it be permanently *negative*, it is of an acid nature; if it be permanently *positive*, it is alkaline. But when he adds that a body may be positive with respect to one body, and negative with respect to another; though nothing is more easy than to reconcile this with the common doctrine of chemical affinity, it would be somewhat difficult to reconcile it with the theory of etectricity. I am far from saying that the thing is impossible; but in order to attempt an explanation of it with success, it would be necessary to make very sweeping changes in the electrical doctrines at present admitted.

Berzelius has given a table of the chemical substances in the order of the intensity of their electricities, beginning with the one attracted most strongly to the positive pole, or the most intensely negative substance; and terminating with the body which is attracted most strongly to the negative pole, and therefore is most intensely positive. The negative intensity diminishes as we proceed downwards in the table, and at last in the centre finally disappears. Then the positive intensity begins; at first very weak, but it gradually increases as we go downwards, and becomes greatest at the lower end of the table. Hence the affinity of the two substances at the two extremes of the table is greatest of all; and as we advance to the middle of the table, that affinity gradually diminishes, and at last disappears. As I do not know the data from which Berzelius constructed his table, I have no means of judging of its accuracy; but I shall give the table in this place, as being well entitled to the attention of the chemical reader. It is doubtless susceptible of great improvements: but such a table, if it could be constructed with accuracy, would be of prodigious service to the progress of scientific chemistry.

Oxygen,
Sulphur,
Nitricum,
Muriatic radicle,
Phosphorus,
Fluoric radicle,
Boron,
Carbon,
Hydrogen,
Arsenic,
Chromium,
Molybdenum,
Tungsten,
Antimony,
Tellurium,
Silicon,
Columbium,
Titanium,
Zirconium,
Osmium,
Bismuth,
Iridium,
Platinum,
Gold,
Rhodium,
Palladium,
Mercury,
Silver,
Lead,
Tin,
Nickel,
Copper,
Cobalt,
Uranium,
Zinc,
Iron,
Manganese,
Cerium,
Yttrium,
Glucinum,
Aluminium,
Magnesium,
Calcium,
Strontium,
Barytium,
Sodium,
Potassium.

According to this table, oxygen and potassium have the greatest affinity for each other; and there is very little affinity between iridium, platinum, and gold.

M. Oersted published at Berlin, in 1812, a work entitled Considerations on the Physical Laws of Chemistry deduced from the new Phenomena. Like Davy and Berzelius, he has adopted the electrical theory of affinity; but he has not been so reserved as these philosophers. On the contrary, he has pushed matters as far as they will go, and has endeavoured to make his electrical hypothesis complete in all its parts. As this theory has attracted great attention, and gained great eclat in Germany, though I believe it is entirely unknown in Britain, I shall give the outlines of it in this place.

He considers the phenomena of electricity, galvanism, magnetism, heat, light, and chemical affinity, as all depending on the same forces; and he shows that the same cause, which in one case produces electrical actions, occasions in another chemical actions. These actions are produced by two forces; the one *negative*, the other *positive*. These forces are opposite to one another; and by being made to act against one another, may suspend or destroy one another.

Heat is produced by the extinction of the two forces, either in electrical or chemical processes. We may suppose also that the light is derived from the same cause.

Acids which are attracted to the same pole as oxygen possess the same force with that principle; while alkalies and combustible bodies, which are attracted to the opposite pole, possess the opposite force. Oersted arranges chemical substances under two series: the first containing the products of combustion; the second, the supporters and combustibles. Those of the one series, according to him, do not combine with those of the other; except sulphur and phosphorus, which combine both with the metals and alkalies, and therefore constitute, as it were, the *transition* from the one series to the other. He endeavours to distribute the bodies in these series in a kind of arithmetical progression, beginning with the most combustible bodies, as hydrogen, ammonia, potassium; and continuing to the least combustible, as platinum, rhodium, iridium. If we suppose the series continued farther, we are led to the idea of a body absolutely incombustible. This body must possess in the highest degree the properties which are the opposite to combustibility, and in consequence of the attraction which exists between it and combustible bodies, it must produce the strongest action, which is that of combustion. This incombustible body, in the present state of our knowledge, is *oxygen*.

Between oxygen and iridium the author places carbon, phosphorus, and sulphur. This last substance, though heat makes it pass into the state of a combustible, ought to be considered as a negative body.

The products constitute a similar series, which commences with the most energetic alkalies, and passes to those which are more feeble, till we come to bodies such as alumina, in which that property is balanced by the opposite property having the same force. Then come bodies of a predominating acidity. This is at first feeble in the bodies nearest the point of equilibrium, but it becomes of an extreme activity in the bodies at the greatest distance from that point. We see that each series commences with a certain sum of a property, which diminishes in arithmetical progression, and terminates by leaving the bodies possessed of the opposite property.

Two bodies which belong to the opposite extremes of a series combine with great force; but the energy of this action seems to change their state, since the product of the combination no longer belongs to the same order of affinities. The compounds of oxygen and a combustible body pass into the series of products, as likewise do those of hydrogen and sulphur, of tellurium, and without doubt those of various other bodies. The compound of an acid and alkali no longer belongs to the series of products, but to that of the salts. On the other hand, compounds between bodies at no great distance from each other, as between two acids or two alkalies, do not go out of the series.

This law embraces all the bodies having the same preponderating forces; so, however, that these bodies cannot be confounded. It presents, says the author, under a simple expression this truth, that the same force may exist in a state so different that all its attraction for the opposite force will not be sufficient to make it enter into combination. The two most different states are found in the supporters and the products. The third state is in the neutral salts; but they bear a close resemblance to those products that are placed near the point of equilibrium. Combustibility, then, is the preponderance of the positive force in a particular state, which M. Oersted calls the *state of supporters*, or of the first class of bodies. Alkalinity presents the same force, but in a different state, which he calls the *state of products*, or of the second class. We do not know the nature of this difference, but we know that it exists, and that the forces are in a state of greater liberty in the first class than in the second.

Combustion sometimes gives us alkaline products, sometimes acid, and sometimes neutral ones. The product, by combining with oxygen, loses either in part or entirely its free positive force, and passes to the order of bodies of the second state. The same thing happens to the negative force of the oxygen. If the body be combustible in a high degree, and does not combine with too much oxygen, the positive force still preserves the preponderance, and the body is alkaline. If, on the contrary, the body be but little combustible, and combine with a great deal of oxygen, the negative force remains preponderant, and the body is acid.

In oxygenated bodies we find of necessity a combustible body and oxygen united together, and constituting a body of the second class. Hence we must conclude that these bodies unite together the two opposite properties, the alkaline and acid, one of which is often rendered insensible by the other. However, in a great number of other combinations, both properties subsist together. This is the case in the oxides of lead, copper, &c. In some bodies we find the positive or negative force in both states at the same time. Thus ammonia possesses both the positive force of the first class and that of the second; while the nitric and oxymuriatic acids possess the negative force both of the first and second class. This is the natural effect of a combination, either feeble or far from the point of saturation, by which a force is reduced to an inferior state. In the combination of tellurium with hydrogen, the negative force of the metal is sufficiently changed by the positive force of the hydrogen to occasion a state of acidity. The positive force contained either in the hydrogen or in the tellurium has not suffered enough of change to come to the state of acid. Hence we find in telluretted hydrogen gas a strong combustibility, and a very distinct acidity, existing together.

It is well known that electricity produces heat. M. Oersted ascribes this effect to a union between the two opposite electric forces; and it is so much the more intense, the greater obstacles the transmission of the electricity meets with, provided that these obstacles may be overcome. The transmission of electricity consists in a series of attractions and repulsions, or in the undulatory movement experienced by the peculiar forces of the body. Chemical union between electro-negative and electro-positive bodies excites always heat, and none of the electrical forces escapes in a state of liberty. However, the increase of conducting power may counterbalance this effect, and even overcome it, and then cold is excited.

There exists three principal combinations between the electrical forces, which are the same as the chemical forces. The first is the combination between the forces themselves; the result of it is the contraction of the two forces, or their reduction to a smaller volume, with the disengagement of light and heat. The second is the combination of a product with a supporter. The result of it likewise is condensation, and the evolution of light and heat, though not to so great an extent as in the first case. The third degree consists in the combination of an alkali with an acid. It is rarely accompanied by the disengagement of light, but always with the disengagement of heat. A condensation likewise takes place unless peculiar causes modify the result. Expansion, which is the effect of a repulsive force in bodies, is most frequently owing to an excess of one of the electric forces. Contraction is the effect of an equilibrium between the forces, and of their mutual extinction.

The forces which produce the electrical and chemical actions of bodies are the same as those which produce the mechanical properties of bodies. Impenetrability depends on the resistance which the expansive power of two forces opposes to a body endeavouring to penetrate the space already occupied by another body. Cohesion is the effect of the two forces, which attract one another. Universal attraction consists in the action at a distance of the two forces upon each other, supposing the expansive power of each force not to extend beyond the surface of bodies.

Such is an abstract of M. Oersted's hypothesis, as far as I have been able to make it out. It is fair, however, to state, that I have not had an opportunity of perusing his own work, but only the abstracts of it published in some of the German journals, and an outline given by Van Mons in his translation of Sir H. Davy's Elementary Work. Hence it is very possible that I may not have done the author justice, and that some of the parts of his hypothesis, which appear the most whimsical and absurd, would assume another aspect under the explanations of the author himself. On that account I shall not attempt any critical examination of this hypothesis, which has given the author considerable celebrity in Germany. Its weak parts and inconsistencies are sufficiently obvious to those who have followed the late improvements in electricity and chemistry. The metaphysical part I do not fully understand; nor have I been able to make out whether the author's electrical forces be substances or qualities.

At present, then, it seems to be the prevailing opinion of chemists that chemical affinity is identical with electrical attraction. The opinion possesses much plausibility, and even probability; but much remains to be done before it can be considered as established, and made the foundation of our chemical reasonings.

II. *Proportions in which Bodies combine chemically.*—That the ultimate particles of matter consist of *atoms*, incapable of farther subdivision, is an opinion which has been pretty generally received among philosophers ever since the time of the Greeks; and since the establishment of the Newtonian philosophy this opinion has become almost universal. That substances always enter into chemical combination, in determinate proportions which never vary, has been known ever since chemists acquired the art of analysing bodies. Thus carbonate of lime, wherever, or in whatever state, it occurs, is always a compound of 43·2 carbonic acid and 57·8 lime; and sulphate of barytes, of 34·5 sulphuric acid and 65·5 barytes. In like manner, the yellow oxide of lead is always a compound of 100 lead and 7·7 oxygen; and red oxide of mercury, of 100 mercury and 8 oxygen. Sulphuric acid is always composed of three parts of oxygen and two parts of sulphur; and carbonic acid, of 2000 oxygen and 751 carbon. This law is universally admitted by chemists; and, indeed, the more rigorously it has been examined, the more conspicuous and decided have become the proofs in its favour. Even Berthollet, who seems to be an enemy to the atomic theory in the abstract, has admitted that all known compounds unite in determinate proportions; and has endeavoured to reconcile this fact to his own opinions by several highly ingenious, and some rather whimsical, arguments. The few exceptions which he was able to muster up against the law have all disappeared before the more rigid and exact examination of modern analysts.

Mr. Dalton was the first person who ventured to account for this fixedness in chemical proportions. According to him, it is the atoms of bodies that unite together. One atom of a body, *a*, unites with one atom of a body, *b*, or with two atoms of it, or with three, four, &c. atoms of it. The union of one atom of *a* with one atom of *b* produces one compound, the union of one atom of *a* with two atoms of *b* produces another compound, and so on. Each of these compounds, of course, must consist of the same proportions, because the weight of every atom of the same body must of necessity be the same.

We have no means of demonstrating the number of atoms which unite together in this manner in every compound; we must, therefore, have recourse to conjecture. If two bodies unite only in one proportion, it is reasonable to conclude that they unite atom to atom. Hence it is most likely that water is composed of one atom of oxygen and one atom of hydrogen; oxide of silver, of one atom silver and one atom oxygen; and oxide of zinc, of one atom zinc and one atom oxygen.

When a body has the property of uniting with various doses of oxygen, we can then determine the number of atoms which constitute the compounds. Thus manganese unites with four doses of oxygen; and supposing the manganese to be represented by 100, the oxygen of each respective oxide is represented by the numbers 14, 28, 42, 56; but these numbers are to each other as the numbers one, two, three, four. Hence the first oxide is composed of one atom manganese and one atom oxygen; the second, of one atom manganese and two atoms oxygen; the third, of one atom manganese and three atoms oxygen; and the fourth, of one atom manganese and four atoms oxygen. In like manner, as mercury combines with two doses of oxygen, and forms two oxides, the first composed of 100 mercury and four oxygen, and the second of 100 mercury and eight oxygen, it is obvious that the first must be a compound of one atom mercury and one atom oxygen, and the second of one atom mercury and two atoms oxygen.

Nor is there any difficulty with respect to iron. There are two oxides of that metal: the first composed of 100 iron and 28 oxygen; the second of 100 iron and 42 oxygen. Now as 28 is to 42 as two to three, it follows that the first is a compound of one atom iron and two atoms oxygen; the second, of one atom iron and three atoms oxygen. The same rule holds good with respect to the oxides of nickel and cobalt.

If we know the number of atoms of which a body is combined, and the proportion of the constituents, there is no difficulty in determining the proportional weight of the atoms of which it is composed. Thus if water be composed of one atom of oxygen and one atom of hydrogen, and if the weight of the oxygen in water is to that of the hydrogen as 7½ to one, then it follows that the weight of an atom of oxygen is to that of an atom of hydrogen as 7½ to one. If black oxide of mercury be composed of one atom of mercury and one atom of oxygen, and if it be composed of 100 mercury and four oxygen, then an atom of mercury is to the weight of an atom of oxygen as 100 to four, or as 25 to one. If black oxide of iron be composed of one atom iron and two atoms oxygen, and if it consist of 100 iron and 28 oxygen, then an atom of iron is to an atom of oxygen as 100 to 14, or as 7·142 to one. Such is the method of determining the weight of an atom of the different substances upon which experiment has hitherto been made. The advantage of such a knowledge is immense; because it gives us the proportions in which the different substances unite together, and even enables us to calculate the proportional constituents of all compound bodies, independent of experiment, and with more accuracy than would result from experiments unless conducted with uncommon precautions.

Hitherto the only persons who have written upon the subject of chemical atoms are Mr. Dalton, Sir Humphry Davy, Dr. Berzelius, Dr. Wollaston, and myself. Mr. Dalton made choice of hydrogen as his unit, because it is the lightest of all the atoms; and Sir H. Davy has followed his example. But as oxygen enters into a much greater number of compounds than any other body, it was chosen by Dr. Wollaston and Dr. Berzelius as the most convenient unit; and in the tables of atoms which I have published in the different volumes of the *Annals of Philosophy*, I have followed their example. Berzelius considers an atom of oxygen to weigh 100, Wollaston makes it weigh 10, and I myself make its weight one. The reader will perceive that these three numbers are the same, the only difference being the position of the decimal point.

The person who has hitherto made the greatest number of experiments upon this important subject is Dr. Berzelius; and he has considered himself as entitled, by the results which he has obtained, to establish two propositions which he considers as axioms or chemical first principles, and which have a prodigious influence on the whole doctrine. These axioms are the following :—

1. In all compounds of inorganic matter one of the constituents is always in the state of a single atom. According to this axiom, no inorganic compound is ever composed of two atoms of *a* united with three atoms of *b*, or of three atoms of *a* united with four atoms of *b*, &c.; but always of one atom of *a* united with one, two, three, four, &c. atoms of *b*. This axiom, if it hold good, which Berzelius thinks it will, greatly simplifies the doctrine of atomic combination, as far as inorganic bodies are concerned, and reduces the whole to a state of elementary facility.

2. When an acid unites to a base, the oxygen in the acid is always a multiple of the oxygen in the base by a whole number, and generally by the number denoting the atoms of oxygen in the acid. Thus sulphuric acid contains three atoms of oxygen: 100 parts of it contain 60 oxygen; and 100 parts of sulphuric acid combine with, and saturate, a quantity of base which contains 20 oxygen. Now 20 multiplied by three, the number of atoms of oxygen in sulphuric acid, makes 60 the quantity of oxygen in 100 of sulphuric acid.

Such are the two axioms of Berzelius, which he has made the foundation of his whole reasoning, and from which he has deduced his rules for determining the proportion of oxygen in bodies, and the number of atoms of which they are composed. If they hold good, and hitherto they have answered wonderfully well, they must be admitted to be of the utmost importance, and to give a facility and elegance to our chemical investigations which could scarcely have been looked for.

Mr. Dalton, the founder of the atomic theory, has not adopted either of these axioms. At the same time he has not advanced any fact in opposition to them; but only that there is nothing in the atomic theory which necessarily leads to their adoption. This is doubtless true. The axioms are merely empyrical, and deductions from analyses. Yet if they hold in all the analyses hitherto made, we cannot well refuse them a good deal of generality; and the best mode of proceeding seems to be to admit them till some exception to them be discovered.

Berzelius, considering the atomic theory to labour under difficulties, which in the present state of our knowledge we are not able to surmount, has substituted in its place another, which he conceives to be easier and simpler. This may be called the theory of volumes. He conceives bodies to be all in the gaseous state, and embraces the opinion of Gay-Lussac, that gaseous bodies always unite in volumes that are aliquot parts of each other. One volume of one body always unites with one, two, three, &c. volumes of another. How this alteration, which consists merely in the substitution of the word *volume* for *atom*, simplifies the atomic theory, or removes any of the difficulties under which it labours, is, I own, beyond my comprehension. But Berzelius has deserved so well of chemistry, that he may be indulged in any innocent whim which produces no deterioration.

I should take up too much room were I here to give a table of the weights of the atoms of bodies. I must satisfy myself with referring to the different papers which I have inserted in the *Annals of Philosophy* on the subject, to the paper of Berzelius in the third volume of the *Annals*, in which will be found his table of the weights of an atom of the simple substances, and to Dr. Wollaston's scale of chemical equivalents. The weights given in these three different tables do not always coincide with each other; but in general a very near approach to coincidence will be perceived. In some cases the weights that I have assigned are half those given by Berzelius. The reason of this is obvious; and the circumstance can occasion no difficulty or ambiguity.

II. *Light and Heat.*

In the account of the progress of chemistry which I gave at the beginning of last year, I had to state a considerable number of important additions to the doctrine of heat, and its connection with light; but at present this department of the science is nearly barren. M. Berard has repeated and confirmed the experiments of Dr. Herschel on the heating power of the different rays of solar light. He found the greatest heating power at the extremity of the red ray. He likewise repeated the experiments of Wollaston, Ritter, and Böckman, on the deoxidizing power of the solar rays. He found it greatest, as they had done, in the violet ray; and traced it in a diminishing rate to the middle of the spectrum, where it disappeared.

Morichini, a Roman chemist, announced some time ago that, when steel needles are exposed to the action of the violet ray, they are converted into magnets. This experiment has been repeated in France, but has not been attended with success.

III. *Simple Supporters and Combustibles with their Compounds.*

There are a considerable number of facts to relate respecting this branch of chemistry.

1. *Iodine.*—This singular substance was discovered some years ago by M. Courtois, a saltpetre manufacturer in Paris. It was first examined by Clement and Desormes, then by Sir H. Davy, and lastly by Gay-Lussac. It is obtained from kelp by a very easy process, which has been described in the *Annals of Philosophy*. French kelp yields it in much greater abundance than British kelp. Hence it would appear that the sea plants that yield it are more abundant in the English Channel than on the east or west coast of Britain. Iodine is in small crystals, which Dr. Wollaston has ascertained to be octahedrons. It has the metallic lustre, and resembles plumbago in colour, though its lustre is considerably

greater. It has a peculiar smell, is very volatile, and very poisonous in its nature when taken internally. Its specific gravity is rather less than four, that of water being one.

Iodine, as far as experiment has hitherto gone, must be considered as a simple substance; and it belongs to the class of supporters, though it is by far the worst supporter known. Its vapour supports the combustion of potassium, and it combines rapidly with phosphorus, evolving much heat, though no light. When iodine is heated, it is volatilized at rather a low temperature, and converted into a beautiful violet-coloured vapour, from which it has received its name. This vapour, as far as I can find, possesses very little elasticity at the temperature of 212°.

Iodine combines with chlorine, and forms a peculiar acid. It does not appear to combine with oxygen. With hydrogen it unites, and forms an acid very much resembling muriatic acid in its smell, though I consider it as rather more pungent than that of muriatic acid. It unites with sulphur, phosphorus, and the metals; and forms a class of bodies, analogous to the oxides, called *iodes*. Some of these possess the properties of acids. It combines with alkalies and earths, and forms with them two classes of salts. The first, consisting of iodine and the base, may be considered as analogous to the iodes; the second, consisting of iodine, oxygen, and the base, have been called *oxiodes*, and may be considered as analogous to the hyper-oxymuriates. It scarcely possesses the property of separating carbonic acid from the bases, and cannot therefore be united with the carbonates so as to form salts; though these bodies readily dissolve a portion of iodine. An atom of iodine weighs about 12·5.

2. *Chlorine.*—Chlorine has the property of combining with two different proportions of oxygen, and of forming two new acids, which have received the names of *chloric* and *chlorous* acids. The first was discovered by Gay-Lussac; the second, by Sir H. Davy. The Germans have given to chlorine the name of *halogen*.

It is scarcely worth while to notice the experiments of the Dutch chemists L. A. Von Meerten and S. Stratingh on this gas, as I do not perceive any thing new in them. Meerten says that chlorine has the property of converting sulphurous acid into sulphuric acid, and nitrous gas into nitric acid. These were the first two experiments that I tried; when Davy published his paper to show that chlorine is still an undecomposed substance; and I soon procured the fullest evidence that neither of these gases is altered by pure chlorine; but when chlorine contains a mixture of common air, which it usually does, it renders nitrous gas yellow in an instant. Meerten found likewise that ether burns in chlorine gas. This discovery was made many years ago by Cruickshank, and published by him in the last volume of Nicholson's quarto Journal, and by myself in the second and subsequent editions of my System of Chemistry; nor is there any thing new with respect to the burning of the metals in chlorine gas, as far as tried by these chemists.

Christian Frederick Bucholz made a set of experiments, in 1812, to determine the quantity of oxygen which can be obtained from hyper-oxymuriate of potash. His experiments were not attended with much success; but he ascertained that a red heat is necessary to drive off this gas. I have myself repeated this experiment more than once, and obtained a result which approached very near to that previously established by the experiments of Chenevix.

3. *Fluorine.*—Sir H. Davy has published several papers upon this hypothetical basis of fluoric acid; but all attempts to obtain it in a separate state have hitherto failed. Indeed, supposing it to exist, its action upon all other bodies seems necessarily to be so violent that there can be little hopes entertained of ever procuring it except in a state of combination.

4. *Azote.*—The two laws of Berzelius relative to chemical proportions do not hold when applied to the combination of azote with oxygen. The reason of this he conceives to be, that azote is not an element, but a compound of oxygen and an unknown base, to which he has given the name of *nitricum.* He has calculated from his theory the quantity of oxygen which azote must contain; and he shows that when this correction is made, the nitrates, as well as other bodies, come under the dominion of his two laws.

Mr. Miers, of London, had been of opinion for several years that azote is a compound of oxygen and hydrogen, and that the experiments of Girtanner were not so inaccurate as has been supposed. In a paper published in the *Annals of Philosophy*, vol. iii. p. 364, he shows that the supposition, that it is a compound of one atom oxygen and six atoms hydrogen, will tally exactly with the atomic theory, and give the weight of the different atoms into which azote entered the very same as they are at present, supposing azote to be a simple substance. This ingenious paper was sufficient to show us that the opinion of Mr. Miers was neither impossible nor improbable. It was not, however, sufficient to determine the opinions of chemists in favour of an hypothesis of so much importance, that the consequence of admitting it would be an almost entire change in the notions at present entertained respecting chemical combination.

Mr. Miers, sensible of the necessity of direct experimental proof in order to give currency to an opinion of such magnitude, has had recourse to direct experiment, and has published a very curious and valuable paper on the subject in the *Annals of Philosophy*, vol. iv. p. 180 and 260. His object in these experiments was to deprive water of a portion, but not the whole, of its oxygen, and thus to convert it into azote. The experiments of Girtanner were directed to precisely the same view. It occurred to Mr. Miers that sulphureted hydrogen gas would probably answer the purpose. Accordingly he passed a mixture of vapour of water and sulphureted hydrogen gas through a copper tube. In one experiment the whole gas that came over possessed the properties of common air, and was a mixture of 80 azote and 20 oxygen. In another a gas was formed, which Mr. Miers considered as sulphureted azotic gas. In a third there was formed an acid gas resembling sulphureted hydrogen in smell, but possessing very different properties. Water absorbed twice its bulk of it. With potash it formed a black insoluble compound, not decomposed by any acid. Such were the different results obtained by Mr. Miers. They are highly curious and interesting; but it is obvious that they require to be followed farther before they can be considered as establishing the compound nature of azote, and that it is composed of oxygen and hydrogen. The inconsistency of the results with each other, the new facts brought to view by every repetition of them; and, above all, the circumstance of the gas, in the most decisive of all the experiments, possessing the properties and composition of atmospherical air, lead to suspicions which require to be obviated. It would be requisite, likewise, to account for the sulphur of the sulphureted hydrogen, and to show that the copper tube can have no influence on the decomposition of this gas. I hope, therefore, Mr. Miers will resume his experiments, and prosecute them till he obtains results free from all such anomalies, and leading to conclusions that cannot be controverted. The investigation is indisputably an object of importance; and he has made such progress in it, that he ought to enjoy the reputation that would infallibly result from so interesting a discovery.

5. *Phosphorus.*—The facts respecting phosphorus, published by Thenard in the Annales de Chimie, had been almost all anticipated long ago by Proust. Hence I conceive it to be unnecessary to detail them here. Heinrich, in his treatise on the phosphorescence of bodies, has stated some facts respecting the temperature at which phosphorus burns in various circumstances, which perhaps may be worth transcribing. When phosphorus is put into the bottom of a narrow glass tube, it may be heated to 482° without taking fire. In the open air phosphorus burns at 99°, and in oxygen gas at 72°. I cannot avoid observing that these determinations are far from precise. Indeed, nothing definite can be established respecting the combustion of phosphorus, because the degree at which it catches fire depends upon its purity. Pretty pure phosphorus I found did not begin to burn rapidly till heated to the temperature of 148°; but if you keep it long in the temperature of 99°, its temperature gradually increases by its slow combustion, and it will, after a certain time, burn rapidly. According to Heinrich, a compound of equal parts phosphorus and sulphur becomes luminous at 30°.

6. *Ammonia.*—The important experiment of Berzelius, who converted mercury into an amalgam, by causing the galvanic battery to act upon it when in contact with ammonia, has not yet been cleared up in a satisfactory manner. It follows from it that ammonia contains a substance of a metallic nature as its base, and that when this base is deprived of oxygen by the influence of the galvanic battery the metal amalgamates with mercury. On the other hand, the analysis of ammonia by means of electricity, and the resolution of it into hydrogen and azote without the least trace of

oxygen, is equally decisive. These two sets of experiments seem at first sight incompatible with each other, and show us that there is something connected with the nature of azote still unknown. The fact that no substance constitutes a saline base, or is capable of uniting with acids, and forming salts, unless it contains oxygen (ammonia alone excepted), is a strong analogical argument in favour of the existence of oxygen in ammonia. It has accordingly induced Berzelius to embrace that opinion; and I must acknowledge that it is difficult to resist so very general an analogy. The anomaly respecting the composition of ammonia will be removed completely whenever it is proved in a satisfactory manner that azote is a compound of oxygen and hydrogen. This circumstance renders that question of still greater importance than it otherwise would be.

7. *Sulphuret of Carbon.*—The properties and constituents of this singular substance, as determined by Drs. Berzelius and Marcet, were stated in our sketch of chemistry given at the beginning of last year. Since that time Berzelius has published additional observations on the combination of sulphuret of carbon with the bases. (*Annals of Philosophy*, iii. 186.) To these compounds he has given the name of *carbo-sulphurets*. The following table exhibits the colour of the precipitates obtained by mixing different metallic salts with a solution of sulphuret of carbon in potash:—

Muriate of cerium	White, or yellowish white.
Sulphate of manganese	Greenish-grey.
Sulphate of zinc	White.
Permuriate of iron	Dark red.
Submuriate of antimony	Orange.
Muriate of tin	Pale orange, then brown.
Nitrate of cobalt	Dark olive-green, at last black.
Nitrate of lead	A fine scarlet.
Nitrate of copper	Dark brown.
Promuriate of mercury	Black.
Permuriate of mercury	Orange.
Muriate of silver	Reddish brown.

Dr. Brewster has found that sulphuret of carbon exceeds all fluid bodies in refractive power, and that in this respect it even surpasses flint glass, topaz, and tourmaline. In dispersive power it exceeds every fluid substance except oil of cassia, holding an intermediate place between phosphorus and balsam of Tolu.

8. *Potassureted Hydrogen Gas.*—Sementini, of Naples, published, about two years ago, a dissertation on potassium, in which he relates his experiments on potassureted hydrogen gas, which was discovered by Davy during his experiments on the metal of potash. The following are all the facts that I can find in these experiments: 1. Potassureted hydrogen gas is heavier than pure hydrogen, and lighter than phosphoreted hydrogen gas. 2. It takes fire, with a kind of explosion, when it comes in contact with the air emitting an alkaline odour. The explosion is louder when the gas comes in

contact with oxygen or chlorine gases. 3. Electricity causes it to expand, and throws down the greatest part of the potassium. 4. Though kept in contact with water, it does not lose the whole, but only a part, of its potassium. Hence Sementini concludes that hydrogen has the property of combining with two proportions of potassium.

9. *Nitrous Oxide.*—From the experiments of Professor Pfaff, of Kiel, it appears that when the nitrate of ammonia employed to prepare this gas contains sal ammoniac, as is commonly the case, the gas which comes over is the same as that noticed by Proust and Vauquelin, which has a peculiar taste and odour, and acts with violence upon the lungs. Hence the method of preventing the occurrence of this foreign gas is obvious.

9. *Sulphureted Azotic Gas.*—A controversy has been carried on in Germany for some years respecting the existence of this gas. It was first announced by Gimbernat as existing in the mineral waters of Aix-la-Chapelle. Then appeared the analysis of these waters by Dr. Reaumont, and by Messrs. Monheim and Lausberg; the experiments of Westrumb on the same subject; and the attempts of Berzelius and Hedenberg to form sulphureted azotic gas artificially, which were not attended with success. I do not consider it as necessary to enter into the particulars of this controversy. I shall merely state what I conceive to be the result of it. No person has ever succeeded in forming sulphureted azotic gas artificially. The waters of Aix-la-Chapelle seem to contain both azotic gas and sulphureted hydrogen gas. The sulphureted azotic gas of Gimbernat seems to have been nothing else than a mixture of these two gases.

IV. *Metals.*

The most indefatigable experimenter of late years on the metallic oxides is Professor Berzelius. For the greater part of his results I must refer to his dissertation On the Cause of Chemical Proportions, published in the third volume of the *Annals of Philosophy*, where almost the whole of his results will be found; but as the details of the experiments are frequently omitted in that dissertation, and as I am aware, from conversations which I have had on the subject with different young chemists, that some of these details would be very acceptable, I shall take the present opportunity of stating some of those which seem most important in this place.

1. *Oxides of Gold.*—The facility with which gold parts with its oxygen is well known, and every chemist is aware of the impossibility of obtaining the oxides of this metal in a state of purity. Berzelius' method of estimating the quantity of oxygen in the peroxide of gold, which alone can be subjected to direct examination, was very simple and ingenious, but required considerable dexterity. He dissolved a given quantity of gold in aqua regia, evaporated the solution gently to dryness, to get rid of the excess of acid, and then redissolved the salt in water. He now ascertained, with rigid accuracy, how much mercury was necessary to precipitate the gold in

the metallic state; for it is evident that this mercury must have united with exactly the quantity of oxygen which the gold contained. In one experiment, 14·29 of mercury precipitated 9·355 of gold; in another, 9·95 of mercury precipitated 6·557 of gold. According to these experiments, it appears that 15·912 of gold, when in the state of peroxide, contain just as much oxygen as 24·24 of mercury, when in the state of peroxide. But according to the experiments of Sefstrom, whom Berzelius follows, peroxide of mercury is composed of 100 mercury + 7·9 oxygen. Hence he reckons the peroxide of gold a compound of

Gold	89·225	100·000
Oxygen	10·775	12·077
	100·000	

If we consider the red oxide of mercury as composed of 100 metal + 8 oxygen, as I have done in my table inserted in the second volume of the *Annals of Philosophy*, in that case we obtain the composition of peroxide of gold as follows:—

Gold	89·137	100·000
Oxygen	10·863	[illegible]
	100·000	

This last statement agrees better with the experiments of Oberkampf than that of Berzelius. Oberkampf found sulphuret of gold composed of 100 gold and 24·39 sulphur. Now if an atom of sulphur weigh twice as much as an atom of oxygen, it follows that peroxide of gold ought to be composed of 100 gold + 12·195 oxygen.

When muriate of gold is exposed to a moderate heat upon a sand-bath, as long as it gives out chlorine gas it assumes a strong yellow colour, and becomes insoluble in water; or if the heat has not been continued long enough, only the portion of muriate of gold which is unaltered is dissolved, while that which has acquired a strong yellow colour remains undissolved. If heat be applied to this substance, or if it be simply exposed to the light of day, it is decomposed, and converted into metallic gold and permuriate of gold. According to Berzelius, the straw-coloured substance is a promuriate of gold. Heat or light deprives one portion of the protoxide of the whole of its oxygen, this oxygen unites with the remaining protoxide, and, converting it into peroxide, permuriate of gold is again formed. Now he found by experiment that in this case two-thirds of the gold are reduced to the metallic state, while one-third is converted into peroxide; so that the protoxide of gold contains just one-third of the oxygen in the peroxide.

I may just observe, that these conclusions are not quite free from objections. I myself think it not unlikely that no oxygen is present in either of these salts. This, however, does not hinder the calculations of Berzelius from approaching to accuracy; though it were to be wished that experiments on the oxides were made in a less exceptionable manner than by combining them with muriatic acid.

2. *Oxides of Platinum.*—To determine the composition of protoxide of platinum, Berzelius proceeded in the following manner. He dissolved a quantity of pure platinum powder in nitro-muriatic acid, evaporated the solution to dryness, to get rid of the excess of acid, and then exposed the salt upon a sand-bath till all evolution of chlorine was at an end. The salt thus treated had an olive-green colour, and was promuriate of platinum. To ascertain its composition, he decomposed it by a red heat. Ten parts of it thus treated left 7·33 of metallic platinum, while 2·67 parts of chlorine gas made their escape. Now he assumed that the chlorine gas is a compound of muriatic acid and oxygen, and that the 2·67 of it contain just the quantity of oxygen necessary to convert 7·33 parts of platinum into protoxide; but 100 parts of chlorine gas, according to Berzelius, are composed of 100 muriatic acid and 29·454 oxygen; therefore 2·67 contain 0·6075; and protoxide of platinum is composed of

Platinum	92·35	100
Oxygen	7·65	8·287

Berzelius determined the oxygen in the peroxide of platinum in the same way as he did that in the peroxide of gold. He ascertained how much mercury was necessary to precipitate a given quantity of platinum in the metallic state. The result was, that 100 mercury precipitate 48·23 of platinum. He therefore considers the peroxide of platinum as composed of

Platinum	85·93	100
Oxygen	14·07	16·38
	100·00	

But if we reckon the peroxide of mercury to contain 100 metal + 8 oxygen, which I believe to be very near the truth, then peroxide of platinum is composed as follows:—

Platinum	85·773	100
Oxygen	14·227	16·587
	100·000	

Now this gives the quantity of oxygen very nearly double that which was found in the protoxide.

3. *Oxide of Palladium.*—Berzelius could only find one oxide of palladium. He employed the same method to analyse it that succeeded with him in the analyses of the peroxides of gold and platinum. He ascertained how much mercury is necessary to decompose a given quantity of muriate of palladium, and reduce that

substance to the metallic state. He found that 100 mercury are equivalent to 55·6 of palladium. Hence he considers the oxide of palladium as composed of

Palladium	87·56	100
Oxygen	12·44	14·209
	100·00	

This will require a small correction, if we consider peroxide of mercury as composed of 100 metal and eight oxygen.

4. *Oxides of Rhodium.*—The experiments of Professor Berzelius on the oxides of this metal being detailed in the *Annals of Philosophy*, iii. 252, I shall here state merely the results which he obtained. He found three oxides of this metal: the first composed of one atom metal + one atom oxygen; the second, of one atom metal + two atoms oxygen; and the third, of one atom metal + three atoms oxygen: and one atom of rhodium weighs 14·903. Hence the oxides are composed respectively of

	Metal.	
Protoxide	100	+ 6·71
Deutoxide	100	+ 13·42
Peroxide	100	+ 20·13

5. *Tungsten.*—There is hardly any metallic substance that occasions greater difficulties to practical chemists than tungsten, both on account of the want of a good process to obtain its peroxide in a state of purity, and on account of the very high temperature which is requisite to melt the tungsten after it has been reduced to the metallic state. The experiments of the El Luyarts upon wolfram are known to every chemist; as are likewise the elaborate experiments on the same metal of Vauquelin and Hecht. Messrs. Allan and Aikin succeeded in reducing it to the metallic state, and verified its great specific gravity as determined by the Spanish chemists. An elaborate set of experiments on this metal were published some years ago by Bucholz. He ascertained that the methods hitherto employed by chemists for procuring pure tungstate of ammonia do not succeed; and he verified the great specific gravity of this metal, having obtained it in grains of the specific gravity 17·4. Now this is the mean of 17·6 given by the El Luyarts, and 17·2 given by Allan and Aikin. But as I mean very speedily to publish a translation of the experiments of Bucholz in the *Annals of Philosophy*, I do not consider it as necessary to enter into farther particulars respecting them in this place.

6. *Platinum.*—The difficulty of reducing this metal to the malleable state, and its great importance in the construction of chemical vessels, are well known. Hitherto it has only been done for sale in Paris and in London. Parisian platinum is by far the dearest; as far as my experience goes, it serves very well for

chemical purposes. Its specific gravity is in general not so high as our London platinum, though I once had a crucible made in Paris above 21 in specific gravity. M. Leithner, who has the charge of the porcelain manufactory at Vienna, has lately proposed a new method of rendering platinum malleable. It is exceedingly simple, and appears to answer well enough in a small scale, though it is not adapted for the construction of large vessels. It consists in making up the fine powder of pure platinum into a paste with oil of turpentine, and laying it in coats upon paper, allowing one coat to dry before another is applied, and continuing to add coats till the layer of platinum is of sufficient thickness. When this is done upon porcelain, and the vessel afterwards exposed to the temperature of from 14° to 18° Wedgwood, the platinum adheres, and may be burnished. When laid upon paper, and then exposed to a strong heat gradually raised, a sheet of platinum remains, which may be hammered, and converted into any shape that is wanted.

7. *Palladium and Rhodium.*—Vauquelin's method of separating these metals from crude platina, and obtaining them in a state of purity, has been given so lately in the *Annals of Philosophy*, that I do not think it necessary to repeat it here. It is sufficiently complicated; and, as far at least as palladium is concerned, does not seem nearly so easy as the method previously given by Dr. Wollaston by means of prussiate of mercury, though it is possible that Vauquelin's process may yield a greater quantity.

8. *Gilding on Steel.*—Gehlen has tried the following method of gilding on steel, and found it to answer. The steel is to be in the first place polished; the part to be gilt is to be rendered rough by means of nitric acid; the steel is then to be dipped into the solution containing the gold; the gold adheres to the rough part of the steel, and may be burnished.

9. *Phosphuret of Copper.*—If we believe Dobereiner, phosphuret of copper, prepared by Sage's method, contains not only copper and phosphorus, but likewise *calcium.* To this last metal, in his opinion, the analogy of the phosphuret to steel is owing.

10. *Separation of Gold and Silver.*—Professor Schnaubert, of Moscow, has lately made several attempts to separate silver from gold by boiling the alloy in sulphuric acid; this acid dissolves the silver, and leaves the gold. The process, though by no means brought to a state of perfection, promises at present to be attended with success. The great difference between the price of sulphuric acid and nitric acid, which is usually employed for the purpose, renders it desirable that this process, which I conceive originated with Mr. Keir, should be subjected to farther trials.

11. *Zinc.*—From my analysis of *blende*, published in the *Annals of Philosophy*, iv. 89, we may conclude that oxide of zinc is composed of 100 metal + 24·42 oxygen; and sulphuret of zinc of 100 metal + 48·81 sulphur; and an atom of zinc weighs 4·095.

12. *Antimony.*—From my analysis of sulphuret of antimony, (Ibid. p. 95,) it follows that it is composed of 100 metal + 35·556

sulphur. If we suppose it a compound of one atom metal + two atoms sulphur, then an atom of antimony will weigh 11·249.

V. *Re-agents.*

I shall state under this head the result of some experiments undertaken by different persons to determine the best re-agents for detecting the presence of different bodies in solution.

1. *Mercury.*—Professor Pfaff, of Kiel, has made a set of experiments on the best means of detecting mercury in solution, especially when in the state of corrosive sublimate. The following are the general conclusions which he considers himself as warranted to draw from these experiments:—

The experiments hitherto made on the action of sulphureted hydrogen on solutions of mercury, are in contradiction with each other, and of course insufficient to answer the purpose for which they were intended.

Water impregnated with sulphureted hydrogen is the most delicate test of the presence of corrosive sublimate and pernitrate of mercury; for it discovers these salts, though diluted with 40,000 times their bulk of water, and though they do not exceed the quantity of $\frac{1}{70}$ or $\frac{1}{100}$ part of a grain.

The action of this liquid upon solutions of mercury is distinguished from its action on all other metals by this circumstance—if there be present in the solution any peroxide of mercury, the precipitate, which is at first in brownish or blackish flocks, becomes very speedily white. The addition of the smallest quantity of peroxide of mercury will render the black precipitate white, provided it be agitated.

Protoxide of mercury in all cases produces a black precipitate with sulphureted hydrogen.

If a small piece of copper coin be put into the solution suspected to contain mercury, it will be covered with a white coating, or at least with white streaks; which, when rubbed, acquire the metallic lustre. By this method Mr. Pfaff was able to detect the presence of $\frac{1}{70}$ of a grain of corrosive sublimate when dissolved in 20,000 times its weight of water.

2. *Muriatic Acid.*—Mr. Meyer, of Stettin, has made some curious observations on the delicacy of nitrate of silver, pronitrate of mercury, pernitrate of mercury, and the solution of subpernitrate of mercury in water, as tests for the discovery of muriatic acid. He found nitrate of silver the most delicate of these four salts. It detected one part of muriatic acid dissolved (in the state of common salt) in 113,664,000 parts of water; but upon this very dilute solution the other three salts had no effect. A solution of one part acid in 56,832,000 parts of water was not affected by the mercurial salts. A solution of one part muriatic acid in 28,416,000 parts of water was rendered slightly opalescent by the pronitrate of mercury, but not altered by the two other mercurial salts. A solution of one part of muriatic acid in 14,208,000 parts of water

was rendered opalescent, both by the pronitrate and pernitrate of mercury, but not by the third mercurial salt; but this third salt produced a slight opalescence in a solution of one part of muriatic acid in 3,552,000 parts of water.

3. *Arsenic.*—Some discussions have taken place in Germany respecting the best test for white arsenic. Gärtner observed that the formation of Scheele's green, by pouring cuprated ammonia into the liquid containing the white arsenic, does not in certain circumstances take place. Schweigger recommends sulphureted hydrogen, which is certainly a most delicate test of arsenic, in consequence of the fine yellow precipitate which it forms. The employment of nitrate of silver as a test for arsenic does not seem to be known in Germany. This test, first pointed out by Mr. Hume, but much simplified and improved by Dr. Marcet, is certainly very delicate; and, when the precautions suggested by Dr. Marcet are attended to, does not seem liable to ambiguity.

4. *Manganese.*—Pfaff has published a set of experiments on the method of separating manganese from iron, and has shown that all the methods hitherto proposed on the Continent are imperfect. Bergman's process has been long given up by all chemists. Vauquelin's, by means of bicarbonate of potash, he found likewise unsuccessful. The method by means of tartrate of potash is incomplete, because the tartrate of potash-and-manganese is soluble in water. Dr. John's method, by oxalate of ammonia, was likewise unsuccessful. Nor did he succeed better by means of the succinates, benzoates, or phosphates. Mr. Hatchett's method of separating the iron, by means of ammonia, was not tried. I find that by means of it I can very easily procure perfectly pure oxide of manganese. Whether it would succeed equally in forming an accurate separation of manganese and iron for the purposes of analysis, I have not tried; though I think it probable that it might be used for that purpose with advantage, supposing iron and manganese to be the only substances in the solution upon which we operate.

5. *Iron.*—Mr. Porrett has recommended triple prussiate of potash, or *ferrureted chyazate of potash*, as he calls it, as the best method of throwing down iron, and ascertaining its quantity. The precautions necessary to be attended to are the following. The ferrureted chyazite must be pure. There must be no excess of acid in the solution, or as little as possible. All other substances precipitated by the ferrureted chyazate must be previously removed. The ferrureted chyazate must not be added in excess, or as little so as possible. The solution being boiled, and the Prussian blue separated and weighed, every hundred parts of it must be considered as equivalent to 34·235 of peroxide of iron in the solution.

I have no doubt that when no manganese is present the ferrureted chyazate may be employed with considerable accuracy to detect the quantity of iron in any substance under analysis; but as it most frequently happens in mineral analysis that iron and manganese are mixed together in unknown proportions, ferrureted chyazate of potash cannot be employed in such cases till a good method is discovered of separating manganese from iron. Gehlen's method is the best hitherto proposed; though it only answers when the whole of the iron is in the state of peroxide.

I may mention here, for the sake of our British manufacturers of sulphuric acid, that E. W. Martius announced in 1811, in a German journal, that he had found white arsenic in a glass carboy of English sulphuric acid. The arsenic had separated from the acid, and formed a crust in the inside of the glass. As the notice merely states the fact of the separation of white arsenic, I conceive it possible that Martius might have been mistaken. If his statement be accurate, the arsenic must have made its way into the carboy by some odd accident or other; for I think it hardly possible that it should have been mixed with the sulphur before its combustion.

VI. *Acids.*

1. *Formic Acid.*—The reader is probably aware that this acid, which exists ready formed in red ants, was originally discovered by Ray and Fisher; that it was first accurately examined by Margraaf; that Arvidson and Oern published a very complete set of experiments on it in 1777 It continued to be considered as a peculiar acid till Fourcroy and Vauquelin published a set of experiments on it in 1803, and drew as a conclusion from them that it is not a peculiar acid, but a mixture of the acetic and malic acids. This dissertation convinced all the French chemists; and induced me in the second edition of my System of Chemistry to expunge it from the list of acids. But Suerzon having published a new set of experiments on it, in 1805, showed that the premises of the French chemists were insufficient to warrant their conclusions, and that formic acid possessed striking and essential properties which distinguished it from acetic acid. I suggested in my System the propriety of a more rigid examination of the *formates* than had hitherto taken place. This seems to have induced Gehlen to undertake a laborious set of experiments on the subject. He observes in the outset, that if I had been acquainted with the experiments of Arvidson and Oern, and of Richter, on the *formates*, I should probably have been satisfied, without requiring any farther proofs. I regret that it has never been in my power to peruse either of the works alluded to by Gehlen. My knowledge of the experiments of Arvidson and Oern is derived from the account of them given by Keir in his Dictionary, and by Bergman in his Treatise on Elective Attractions. Gehlen's experiments are quite decisive; but they are, unfortunately, too long to be detailed here. He prepared, in the first place, formate of copper, from which he separated the formic acid by distilling it with sulphuric acid in a retort. He compared the pure formic acid thus obtained with acetic acid, procured from acetate of lead by a similar process.

Thus prepared, it has an acid, and peculiar taste and smell, quite different from that of acetic acid. When cooled down sufficiently, it becomes solid, but does not crystallize. Its specific gravity was 1·1168; and, when diluted with its own weight of water, the specific gravity becomes 1·060; and when with twice its weight of water, it becomes 1·0296. In all these respects acetic acid is very different. It likewise requires different proportions of bases to neutralize it. Gehlen describes minutely the formates of copper and barytes, and compares them with the acetates of the same bases. They differ in colour, solubility, form of crystals, and all their other properties, from each other.

2. *Ferrureted Chyazic Acid.*—Mr. Porrett has discovered two new acids, and rendered it probable that many more exist. Ferrureted chyazic acid is obtained from the salt formerly called *triple prussiate of barytes.* This salt is dissolved in water, and as much sulphuric acid added as is just sufficient to neutralize the barytes present. The mixture, being agitated in a phial, is set aside for some time. Sulphate of barytes precipitates, and the ferrureted chyazic acid remains in solution in the liquid. Its properties are as follows:—

It has a pale lemon colour; but no smell. It is decomposed by a gentle heat, or by exposure to a strong light. Prussic acid is then formed, and white triple prussiate of iron, which is soon changed into Prussian blue. It separates acetic acid from all its combinations. It combines with the different bases, and forms the salts formerly called triple prussiates. This acid is composed of four constituents; namely, black oxide of iron, carbon, hydrogen, and azote; or perhaps it would be as well to consider it as composed of five constituents, *oxygen, iron, carbon, hydrogen,* and *azote.* It would be a curious, but very difficult problem, to determine the proportions in which these different constituents are united in this complicated acid. How many atoms of each enters into it. From the great permanency of this acid when compared with some of the vegetable and animal acids, one would be disposed to suspect that its composition is very simple. Perhaps an atom of each constituent would not be very far from the truth.

According to Mr. Porrett, ferrureted chyazate of potash is composed of

Black oxide of iron	17·26	Ferrureted chyazic acid	47·66
Prussic acid	30·40		
Potash			39·34
Water			13·00
			100·00

This analysis cannot be reconciled with the supposition of ferrureted chyazic acid being a compound of an atom of each of its constituents. If we suppose the salt composed of an atom of acid and an atom of base, as is usual with the salts of potash, then an atom of the acid would weigh only 7·268, which is considerably less than an atom of each of the constituents, which together weigh 10·829. If, on the other hand, we take the proportion of black oxide of iron and prussic acid as a criterion, the atom of acid ought to weigh 22·485, and it might be a compound of one iron + one oxygen + six carbon + five azote + six hydrogen; besides which many other numbers might be chosen. These anomalies render Mr. Porrett's analysis of the ferrureted chyazate of potash somewhat doubtful.

Ferrureted chyazate of barytes he found composed of

Ferrureted chyazic acid	34·31
Barytes	49·10
Water	16·59
	100·00

Ferrureted chyazate of iron is composed of

Black oxide of iron	19·83	Ferrureted chyazic acid	53·380
Prussic acid	34·05		
Peroxide of iron			34·235
Water			12·385
			100·000

3. *Sulphureted Chyazic Acid.*—Mr. Porrett discovered this acid in 1808, by boiling three or four parts of Prussian blue in powder with one part of sulphuret of potash, and a sufficient quantity of water. The new acid is gradually formed, and neutralizes the potash in the solution. Various other processes were attended with success. To obtain the acid from this solution in a state of purity, Mr. Porrett employed the following method:—Add sulphuric acid till the liquid acquires a decidedly sour taste: then keep it for some time nearly at the boiling point. When cold, add to it a little black oxide of manganese, which will turn it to a beautiful crimson colour. Filter the liquid, and add to it a solution containing two parts of sulphate of copper and three parts of prosulphate of iron, till the crimson colour disappears. A copious white precipitate falls, composed of protoxide of copper combined with sulphureted chyazic acid. Boil the precipitate in a solution of potash, which separates the acid, and leaves the oxide of copper. Mix the potash solution with sulphuric acid, and distil. The sulphureted chyazic acid comes over into the reservoir. It is still mixed with some sulphuric acid, from which it may be freed by carbonate of barytes.

The acid thus obtained is colourless, has a strong smell, analogous to that of acetic acid, and its specific gravity is 1·022. At a boiling temperature it dissolves a little sulphur. This acid consists of two-thirds of its weight of sulphur and one-third of the constituents of prussic acid. The salts which it forms have the following properties:—

Sulphureted chyazate of potash: a deliquescent salt, soluble in alcohol.

——————— of soda: ditto, crystallizes in rhombs.

——————— of lime: ditto, soluble in alcohol, from which it may be obtained in needle-form crystals.

——————— of ammonia: ditto, not crystallizable.

——————— of magnesia: ditto, when dried has a micaceous appearance.

——————— of alumina: crystallizes in octahedra, which do not deliquesce.

——————— of barytes: a deliquescent salt, crystallizing in long slender prisms of a brilliant white. It is composed of

Sulphureted chyazic acid	30·1
Barytes	69·9
	100·0

——————— of strontian: a deliquescent salt, crystallizing in long slender prisms in groups radiating like zeolite.

——————— of silver: a white insoluble powder.

——————— of mercury: a white insoluble powder.

——————— of potash and prussiate of mercury: a brilliant silvery lustre; very soluble in hot, and little soluble in cold, water.

——————— of protoxide of copper: a white powder, insoluble in water. Its constituents are,

Sulphureted chyazic acid	36·855
Protoxide of copper	63·145
	100·000

——————— of peroxide of copper: a bright pea-green liquid.

——————— of protoxide of lead: a soluble salt in obtuse rhombs.

——————— of protoxide of iron: a colourless and very soluble salt.

——————— of peroxide of iron: a beautiful crimson salt, very deliquescent.

The sulphureted chyazates of tin, bismuth, manganese, zinc, cobalt, nickel, palladium, uranium, molybdenum, and chromium, are very soluble.

4. *Fuming Sulphuric Acid.*—The singular qualities of the fuming sulphuric acid manufactured at Nordhausen, in Germany, from green vitriol, have long attracted the attention of chemists, and various solutions of the anomalies which it presents have been given. Fourcroy's explanation of it, by affirming that it is a mixture of sulphuric and sulphurous acids, has been pretty generally

acceded to; yet it appears that it is not the true one. Mr. Vogel, apothecary at Bayreuth, has lately published a very elaborate set of experiments on this acid, which, as far as they go, appear decisive. The following are the facts which he has established by his experiments: Fuming sulphuric acid contains no sulphurous acid; nor can it be formed by uniting these two acids together, nor by distilling a mixture of sulphur and sulphuric acid. Fuming sulphuric acid attracts no oxygen, nor does it produce any alteration on atmospherical air. When mixed with water, it is converted into common sulphuric acid. When combined with bases, it forms common sulphates. It dissolves some sulphur, and acquires a brown, green, or blue colour, according to the proportion of sulphur which it holds in solution. It combines, likewise, with phosphorus. Vogel considers it as common sulphuric acid united with some imponderable substance, and brought by its means to a more powerful acid state. The direct consequence from his experiments seems to be, that it is sulphuric acid free from water. There is, however, a fact stated by Dobereiner, in a very long dissertation which he has published on the action of the different kinds of sulphuric acid on nitric acid, which, if accurate, would require an explanation. He says that when a mixture of fuming sulphuric acid and nitric acid is heated, the nitric acid is decomposed into nitrous gas and oxygen gas, but no such change is produced by heating a mixture of common sulphuric acid and nitric acid. I do not perceive very clearly how this fact was ascertained. Supposing nitrous gas and oxygen gas to be evolved together out of the liquid, they could not be collected, for they would instantly combine, and form nitrous acid; but supposing the fact correct, I can conceive it to be owing to this circumstance. The fuming sulphuric acid deprives the nitric acid of the whole of its water. Now in this state it is probably much more easily decomposed than when it contains water. Common sulphuric acid will not produce this effect so completely, because it is already combined with an atom of water.

5. *Prussic Acid.*—Mr. Bergeman, apothecary in Berlin, discovered, in 1811, that the bark of the prunus padus contained a notable quantity of prussic acid. Water distilled from this bark proved fatal to animals when taken internally.

6. *Acetic Acid.*—The following very extraordinary experiment was made by Nasse, one of the members of the Imperial Academy of St. Petersburg, and is related by him in a letter to Professor John. Take a glass vessel and fill it with a mixture of equal bulks of carbonic acid gas and common air, and put into it a little water so as hardly to cover the bottom of the vessel. Stop it up, and lay it aside for some months, shaking it occasionally. Then open it, and leave it for some weeks with the mouth slightly covered. Acetic acid will be perceived formed in it, both by the taste and smell. Nasse obtained his carbonic acid by the action of dilute sulphuric acid on Carrara marble. Here is the formation of acetic acid without the presence of any animal or vegetable substance: nothing else

than carbonic acid, common air, and water. The experiment would deserve a careful repetition. If correct, how much light would it not throw on the nature of acetic acid?

7. *Arsenious Acid.*—Various and discordant statements have been published respecting the solubility of white arsenic in water. The result of Klaproth's trials was lately published in the *Annals of Philosophy.* Since that time a still more elaborate set of experiments on the same subject has been published by Bucholz. His results agree more nearly with those of Klaproth than with any other; though there are several anomalies in his experiments which are sufficiently puzzling. I have long been of opinion that the white oxide of arsenic exists in two states; namely, in the state of pure oxide, and in the state of hydrate of arsenic. When first prepared, it is transparent and colourless, like glass; but it gradually becomes white and opake, and puts on the appearance of enamel. The glass I conceive to be the pure oxide of arsenic; the enamel, to be a hydrate. Now if this opinion be well founded, we may expect to find a difference in the solubility of white arsenic in these two states. I think it probable that several of the anomalies are owing to chemists not having hitherto attended to this difference of state.

VII. *Salts.*

This is always one of the most prolific departments of chemistry, on account of the great number of salts, and the importance of being acquainted with their properties; but this historical sketch has already swelled so much, that I shall omit all the salts treated of in the *Annals of Philosophy* during the last year.

1. *Calomel.*—Mr. Jewel's improvement in the manufacture of calomel, by making it pass in the state of vapour into water, is known, I presume, to most of my readers; having been made known to the public by Mr. Luke Howard, in whose manufactory it took place, about four years ago.

2. *Oxalates.*—Vogel, of Bayreuth, has published two very elaborate, and I conceive very accurate, sets of experiments on the analysis of several of the oxalates. I cannot attempt in this place to do more than give a bare table of his results. What makes these experiments more valuable, is their agreement with the views of Berzelius respecting the composition of salts; though when Vogel made his experiments (at least the set of them first published,) it does not appear that he was acquainted with Berzelius' opinions on the subject.

A hundred parts of oxalic acid require for saturation a quantity of base which contains 21·2 parts of oxygen; or in other words, in the neutral oxalates the acid contains three times as much oxygen as the base.

When binoxalate of potash is poured upon carbonate of copper, a solution takes place, and two salts are formed, distinguished from each other by their crystalline shape; the one crystallizing in needles, the other in tables. The constituents of the first of these salts are,

Peroxide of copper	20·50
Potash	25·04
Oxalic acid	36·46
Water	18·00
	100·00

The constituents of the second are,

Peroxide of copper	22·5
Potash	27·0
Oxalic acid	40·5
Water	10·0
	100·0

Thus they differ in their water of crystallization; the one containing double the quantity of the other.

Oxalate of lime is composed of	Acid	49·5	56·25
	Lime	38·5	43·75
	Water	12·0	
		100·0	100·00

Oxalate of potash	Acid	43·06	43
	Potash	56·77	57
	Water	0·17	
		100·00	100

Binoxalate of potash	Acid	55·93	64·02
	Potash	31·44	35·98
	Water	12·63	
		100·00	100·00

Oxalate of soda-and-copper	Acid	46·48
	Soda	19·02
	Peroxide of copper	23·50
	Water	11·00
		100·00

Dry oxalate of soda	Acid	54·77
	Soda	45·23
		100·00

Foliated oxalate of ammonia-and-copper	Acid	47·5
	Ammonia	10·5
	Peroxide of copper	25·0
	Water	17·0
		100·0

Effloresced oxalate of ammonia-and-copper	Acid	36·00
	Ammonia	16·29
	Peroxide of copper	39·00
	Water	8·71
		100·00

Pulverulent oxalate of ammonia-and-copper	Acid	43·00
	Ammonia	9·72
	Peroxide of copper	45·58
	Water	1·70
		100·00

Sulphate of potash-and-copper	Acid	72·15	36·075
	Potash	42·85	21·425
	Peroxide of copper	36·00	18·000
	Water	49·00	24·500
		200·00	100·000

3. *Chromates.*—Dr. John has published a paper on the chromates, in which he describes the properties of twelve chromates hitherto unknown, or nearly so. As this paper is short, and cannot well be abridged, I have inserted a translation of it in the last number of the *Annals of Philosophy*, to which I refer the reader.

4. *Sal-ammoniac.*—Before concluding this department, I may take the opportunity of mentioning that Mr. Trimmer informs me that the sand employed for mixing with the clay by the London brick-makers is brought from below Woolwich. It probably, therefore, contains some common salt, derived from the sea water with which it is washed. This may be the source of the muriatic acid which goes to the formation of the sal-ammoniac that sublimes during the burning of the bricks; but if this be the case, Nature employs during this process some method of decomposing common salt at present unknown to manufacturers; but which it might be well worth their while to endeavour to trace experimentally, unless we suppose the whole acid to come from the muriate of magnesia.

VIII. *Composition of Alcohol and Ether.*

M. de Saussure has given us a new, and seemingly very accurate, analysis of alcohol and sulphuric ether. The constituents of alcohol are as follows:—

Carbon	51·98
Oxygen	34·32
Hydrogen	13·70
	100·00

This result seems to show that alcohol is composed of

	Atoms.
Oxygen	1
Carbon	2
Hydrogen	3
	6

An atom of it, according to this statement, would weigh 2·898. This composition is very simple, considering that the number of constituents of alcohol amounts to three.

The constituents of sulphuric ether are,

Carbon	67·98
Oxygen	17·62
Hydrogen	14·40
	100·00

This result seems to show that sulphuric ether is composed of

	Atoms.
Oxygen	1
Carbon	5
Hydrogen	6
	12

So that, if the analysis of Saussure be correct, sulphuric ether contains twice as many atoms in its composition as alcohol. The weight of an integrant particle of it, according to this statement, would be 5·547.

IX. *Analysis of Waters.*

Bouillon-Lagrange and Vogel have published an elaborate analysis of the sea-water that washes the different coasts of France. The following table exhibits the results of their analyses:—

	Weight.	Residue of Evaporation.	Carbonic Acid Gas.	Common Salt.	Muriate of Magnesia.	Sulphate of Magnesia.	Carbonates of Lime and Magnesia.	Sulphate of Lime.
	Gram.	Gr.	Gr.	Gr.	Gr.	Gr.	Gr.	Gr.
Water of the Channel......	1000	36	0·23	25·10	3·50	5·78	0·20	0·15
Water of the Atlantic......	1000	38	0·23	25·10	3·50	5·78	0·20	0·15
Water of the Mediterranean.	1000	41	0·11	25·10	3·25	6·25	0·15	0·15

Berzelius has published an analysis of a very complicated mineral water in Stockholm. Its constituents per Swedish kanne are as follows:—

	Grains.
Muriate of soda	$21\frac{1}{3}$
Nitrate of potash....................	18
Sulphate of potash	$\frac{1}{7}$
Nitrate of lime	24
Nitrate of magnesia	$4\frac{2}{3}$
Carbonate of lime	15
Sulphuric acid	7
Carbonate of magnesia	$\frac{11}{14}$
Carbonate of iron	$\frac{2}{7}$
Silica	$\frac{5}{7}$
Extractive matter and loss	$\frac{2}{7}$
	$92\frac{3}{14}$

According to the analysis of Mr. Funke, apothecary at Linz, on the Rhine, the mineral water of Tönnestein, on the Rhine, contains the following constituents in five pounds weight of it:—

	Grains.
Carbonate of iron	$\frac{1}{2}$
Carbonate of lime	45
Carbonate of soda	$36\frac{1}{4}$
Muriate of soda	$4\frac{3}{4}$
Sulphate of soda	4
	$90\frac{1}{2}$

Five pounds weight of the mineral water of Heilbrunn contains, according to the same analyst,

	Grains.
Carbonate of soda	54
Sulphate of soda	6½
Muriate of soda	24
Carbonate of lime	55½
Carbonate of magnesia	2
Carbonate of iron	1
	143

The same chemist obtained the following substances from five pounds of the mineral water of Obermenning:—

	Grains.
Carbonate of soda	4
Muriate of soda	3½
Sulphate of soda	4
Carbonate of lime	10
Carbonate of iron	4
	25½

Finally, the mineral water of Heppingen gave the same chemist the following ingredients. He operated, as before, upon five pounds of the water:—

	Grains.
Carbonate of iron, could not be weighed.	
Carbonate of lime	6¼
Carbonate of soda	31
Carbonate of magnesia	12
Sulphate of soda	10¼
Muriate of soda	15
	75

X. *Vegetable Substances.*

The field of vegetable chemistry is so vast, and still so imperfectly explored, that it is always a very prolific department. I must at present confine myself within as narrow limits as possible, and avoid indulging reflections even when they obtrude themselves upon me.

1. *Thibet Caoutchouc.*—This is a reddish elastic substance, which is used as beads in the Mediterranean. I have seen strings of beads repeatedly brought to this country from Malta, and once made a few trials on one of these beads, sufficient to show that it was a vegetable substance, and that it had some resemblance to the oils in its chemical properties. It has been particularly examined by John and Bucholz. Neither alcohol, ether, nor oils, dissolve it completely, though they produce some effect on it. Potash ley dissolves

it, and an acid separates it from the solution in the state of an oil. It dissolves in sulphuric and nitric acids. When heated, it does not melt; but it soon loses its red colour; it seems to me to be very analogous to linseed oil boiled to dryness. Nothing is known respecting its natural history. It is probably a natural production. It is said to be much employed in Thibet. Whether what is used in the Mediterranean comes from Thibet I do not know.

2. *Cajeput Oil.*—This oil has lately acquired considerable celebrity in some parts of England, as a most effectual remedy for rheumatism when applied externally to the diseased part by friction. It was first made known in Europe by the Dutch. According to Murray, it was first brought to Holland about the beginning of the eighteenth century. In 1719 it made an article in the Materia Medica in some of the German Pharmacopœas. It is doubtless an essential oil. Dr. Roxburgh has lately laid a very accurate account of the tree which yields this oil before the Linnæan Society. He cultivated these trees at Calcutta for about ten years; so that the doubts still remaining respecting the nature of the plant from which this oil comes may be considered as removed.

3. *Sugar from Starch.*—Kirchhoff's discovery of the method of converting starch into sugar was no sooner known in Germany than it occasioned a great many publications on the subject, and numerous attempts to improve the process, and render this new sugar a substitute for common sugar. These attempts were natural at a time when the mistaken policy of Bonaparte had shut out Europe from all access to foreign countries, and precluded the supply of common sugar, except at an enormous price. I cannot find, however, that any of these attempts were attended with success; or that much additional chemical information was even added to Kirchhoff's original discovery. This discovery was owing to an accident. In consequence of the war between Great Britain and Russia, the Russians found it difficult to procure gum. Kirchhoff's object was to render starch a substitute for gum. He thought that he would improve it by boiling it in weak sulphuric acid. He gradually lengthened the boiling process, in expectation of rendering the starch more completely gum. The result was, that it acquired a sweet taste, and the properties of sugar. Schrader has shown that by increasing the quantity of sulphuric acid the length of time necessary for boiling may be shortened. Thus five or six parts of sulphuric acid to 100 parts of starch require only six or eight hours boiling. Nasse found that nitric acid and muriatic acid produce this change on starch as well as sulphuric acid. Starch sugar is not so sweet as common sugar; but I have seen it as white, and very like common loaf sugar in appearance.

4. *Sap of the Acer Campestre.*—In the year 1811 Professor Scherer, of Vienna, examined the sap of the acer campestre, or common maple, with a view, I presume, to the sugar which might be extracted from it. This sap, when in small quantities, was

colourless, like water: but when collected in greater abundance, it was imperfectly transparent, and had a milky appearance. It had a sweet taste, and its specific gravity varied. It contained an albuminous substance, which was precipitated in flocks by heating the liquid. There was likewise another substance which precipitated in flocks, that seemed to be different from the albumen; but the most remarkable constituent was a salt, which crystallized sometimes in small prisms, sometimes in plates. It was white, translucent, and had little lustre. 1000 parts of cold water dissolved nine parts of this salt; 1000 parts of boiling water dissolved 17 parts. This salt was composed of lime and a peculiar vegetable acid, bearing some resemblance to the moroxylic acid of Klaproth. Scherer calls it *maple acid.* We might distinguish it, if it be a peculiar acid, by the name of *aceric acid*; for Scherer's name, *feldahorn acid,* cannot with propriety be adopted into other languages.

5. *Conium Maculatum.*—Schrader has subjected *conium maculatum,* common hemlock, to a chemical analysis. The following are the ingredients which he obtained from a thousand grains in weight of the plant:—

Extractive	27·3
Gummy extract	35·2
Resin	1·5
Albumen	3·1
Green fecula	8

He obtained also the following salts from the same quantity of the plant:—

Phosphate of lime	46
Phosphate of magnesia	30·1
Carbonate of lime	27·4
Carbonate of magnesia	20·2
Carbonate of potash	115·0
Sulphate of potash	10·8
Muriate of potash	4·2

6. *Brassica Oleracea Viridis.*—To Schrader we are indebted likewise for a chemical analysis of the *brassica oleracea viridis,* a variety of the sea cabbage. From 1000 parts of this plant he obtained the following substances:—

Extractive	23·4
Gummy extract	28·9
Resin	0·5
Albumen	2·9
Green fecula	6·3

The salts which 1000 parts of this plant yielded were the following:—

Phosphate of lime	71·4
Phosphate of magnesia	8·2
Carbonate of lime	40·1
Carbonate of magnesia	35·6
Carbonate of potash	107·0
Sulphate of potash	73·2
Muriate of potash	15·2

7. *Gums.*—Dr. John has published the result of his experiments on some varieties of gum which exude spontaneously from different trees in Germany.

a. Plum-gum.—The variety of plum known by the name of Myrobala, which is yellow and round, when pricked by insects or chopped, gives out a white clear sap, which gradually hardens into gum. The constituents of this gum are,

Gum arabic	12·5
Cerasin	87·5
	100·0

By *cerasin* Dr. John means a soft gelatinous matter which remains undissolved when the gum is treated with water. It is equally insoluble in alcohol; but water acidulated with sulphuric acid dissolves it in a boiling temperature. This substance has been observed by Vauquelin, and by other chemists. It exists in the cherry-tree gum of this country.

b. Gum from the Stem of the Prunus Avium.—Its constituents, according to John, are,

Cerasin	80
Gum	20
	100

Besides some traces of lime and potash in combination with an acid.

c. Sweet Cherry-Tree Gum.—Its constituents are as follows:—

Gum	97
Phosphate of lime Lime combined with a vegetable acid Potash combined with a vegetable acid	3
	100

I do not exactly know what Dr. John means by the *sweet cherry.* The sweetest cherry which I know is the small black cherry which grows wild abundantly in some parts of Scotland, and in that country is called *geen.* The gum from this tree I have examined, and know that it contains a portion of Dr. John's cerasin. His sweet cherry, therefore, probably is the fruit of another tree.

8. *Starch.*—Some years ago Bouillon Lagrange made the observation that when starch is roasted it acquires the property of dissolving in cold water. Professor Döbereiner, of Jena, has published a set of

experiments on this subject. I perused them with pleasure, because I found them very similar to a set of experiments which I made myself on the same subject ten years ago, part of which were published in Nicholson's Journal, and part in a Report on Malting, Brewing, and Distilling, printed by order of the House of Commons; but as this Report can scarcely be said to be published, as it was never on sale, the facts contained in it have never become generally known.

Döbereiner found that when starch was roasted till its colour became grey, it was only partially soluble in cold water, and the solution was nearly colourless; but when roasted till it became yellow, it dissolved completely in cold water, and the solution was dark coloured. Both these solutions exhibited the same properties when examined by re-agents.

They were precipitated by alcohol. Infusion of nutgalls produced a copious precipitate, which was readily dissolved on heating the liquid, and appeared again when the liquid cooled. Barytes water likewise occasioned a precipitate, which was completely soluble in acetic acid. It was precipitated also by nitrate of mercury; but not by silicated potash, permuriate of iron, nitrate of silver, or corrosive sublimate; nor by nitrate of lead, hydro-sulphuret of potash, or alum. It slowly decomposes nitric acid. When treated with very dilute sulphuric acid, it is not converted into sugar. It does not readily ferment.

Döbereiner likewise made observations on the quantity of starch still to be found in beer; on the difference between the starch of raw grain and of malt. I have not room here to state the particulars; but know from previous experience that the facts which he states are tolerably accurate.

9. *Cucumber.*—Dr. John has subjected the cucumber to a chemical analysis. The following were the constituents which he procured from 600 grains of it:—

Water	582·80
Matter similar to the fungin of mushrooms	3·20
Albumen	0·80
Resin	0·25
Extractive with a sweet substance	9·95
Gluten	3·00
Phosphate of lime	
Phosphate of potash	
Phosphoric acid	
A salt with base of ammonia	
Malic acid united to a base	
Sulphate of potash	
Muriate of potash	
Phosphate of iron	
Aroma	
	600·00

600 parts of the husk of the cucumber contain 90 parts of dry matter; the constituents of which bear a resemblance to those of the interior part.

10. *Potatoes.*—Lampadius has made comparative experiments on four different varieties of potatoe; namely, the Peruvian potatoe brought from America by Humboldt, and planted in Germany; the English potatoe; the onion potatoe;* and the Voichtland potatoe. The following were the proportions of the different constituents yielded by 100 lbs. of the respective sorts of potatoe.

1. *Peruvian Potatoe.*

	lbs.	oz.
Starch	15	0
Fibrous matter	5	8
Albumen	1	28
Extractive	1	28
Water	76	0
	100	0

2. *English Potatoe.*

	lbs.	oz.	dr.	gr.
Starch	12	29	1	2
Fibrous matter	6	26	2	4
Albumen	1	1	1	2
Extractive	1	22	2	4
Water	77	16	1	48
	100	0	0	0

3. *Onion Potatoe.*

	lbs.	oz.	dr.	gr.
Starch	18	24	0	0
Fibrous matter	8	12	0	0
Albumen	0	28	0	0
Extractive	1	21	1	2
Water	70	10	2	58
	100	0	0	0

4. *Voichtland Potatoe.*

	lbs.	oz.	dr.	gr.
Starch	15	13	1	2
Fibrous matter	7	4	0	0
Albumen	1	8	0	0
Extractive	1	30	2	4
Water	74	8	0	54
	100	0	0	0

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* I do not know the variety which is known in Germany by the name of *Zwieb el Kartoffel.*—T.

It is not easy to say what kind is meant by the name of English potatoe. There are many varieties in England, differing very much in their properties. In general the potatoes raised in the north of England are better than those raised in the south; though to this rule there may be exceptions.

XI. *Animal Substances.*

Dr. Gordon's experiments showing the evolution of heat during the coagulation of the blood (*Annals of Philosophy*, iv. 139,) constitute a pleasing fact, because they confirm the general law of the evolution of heat when bodies change from a liquid to a solid form. Dr. Prout's farther experiments on the quantity of carbonic acid evolved by respiration at different times of the day, and as affected by various circumstances, and the confirmation they have received from the recent experiments of Dr. Fife, are deserving of the attention of physiologists, and are well calculated to throw additional light on this still mysterious function, so essential to life, that it cannot be interrupted; yet, as far as we can find, serving merely to remove a little carbon from the blood.

1. *Air in the Swimming Bladders of Fishes.*—All chemical readers are acquainted with Biot's experiments to determine the composition of the air in the swimming bladders of fishes, published in the Memoirs d'Arcueil, and with the unexpected result that the proportion of oxygen in these bladders increases with the depth at which the fish live. Since the publication of that paper an elaborate set of experiments on the same subject has been published by Configliachi, an Italian Professor. He has confirmed and extended the facts ascertained by his predecessors. One of the most curious parts of his paper is a table of the proportion of oxygen gas found by him in sea-water of different depths. The following is a copy of that table:—

Depth in Metres.	Proportion of Oxygen.
50	28·7
100	28·8
150	28·5
200	27·9
250	28·4
300	28·7
350	29·0
400	28·5
450	27·8
500	28·1
550	28·4
600	28·3
650	28·3
700	28·2

2. *Urine.*—Professor Wurzer has published a set of experiments

on a remarkable urine emitted by a man of the age of 33. He had been afflicted with a gonorrhæa virulenta ever since the age of 24. In consequence of cold, to which he had been exposed, his breasts swelled, and a few days after he passed the remarkable urine which drew the attention of Professor Wurzer. It was milk-white, and contained in it a notable quantity of a matter which possessed exactly the properties of *curd*.

3. *Magnesia in Human Bones.*—Fourcroy and Vauquelin first discovered the presence of magnesia in the bones of inferior animals; but they could detect none in human bones. For this they assigned a physiological reason. In man the magnesia is carried off in the urine; but this is not the case in the inferior animals. Hence it appears in the bones of the latter, but not of the former. Berzelius repeated the experiments of Fourcroy and Vauquelin, and could not by their method detect any magnesia in human bones; but he gives another process, by which he assures us he detected it. Professor Hildebrandt, of Erlangen, has lately resumed this subject, and repeated the experiments of Fourcroy and Vauquelin. Like them, he could detect no magnesia in human bones, and therefore concludes that Berzelius was mistaken. Now I must acknowledge that this mode of proceeding appears very extraordinary. To repeat a process, which, Berzelius himself admits, does not yield any magnesia, and then to conclude that Berzelius is mistaken because the experiment turns out just as he foretold it would. To refute Berzelius' conclusion, it would be necessary to repeat the experiment as he describes it, or to show experimentally that the experiment is incapable of repetition.

4. *Urinary Calculus of a Horse.*—A urinary calculus of a horse analysed by Professor Wurzer was found to contain the following constituents:—

Carbonate of lime	66
Phosphate of lime	20·05
Carbonate of magnesia	4·06
Red oxide of iron	0·005
Animal matter	9·895
	100·000

5. *Milk.*—In 1813 C. F. Schwarz published an inaugural dissertation at Kiel on the analysis of milk. From 1000 parts of cow's milk he obtained the following substances:—

Phosphate of lime	1·805
Phosphate of magnesia	0·170
Phosphate of iron	0·032
Phosphate of soda	0·225
Muriate of potash	1·350
Lactate of soda	0·115
	3·697

1000 parts of human milk contain,

Phosphate of lime	2·500
Phosphate of magnesia	0·500
Phosphate of iron	0·007
Phosphate of soda	0·400
Muriate of potash	0·700
Lactate of soda	0·300
	4·407

Such is as concise a sketch as I have been able to draw up of the improvements which chemistry has undergone of late years, chiefly in those countries with which we have had the least intercourse. I have omitted some important facts altogether, because I intend to insert translations of the papers containing them in the succeeding numbers of the *Annals of Philosophy*, as speedily as I can find room for them.

II. Mineralogy.

This branch of science is divided into two parts; namely, Oryctognosy and Geognosy. It will be convenient to take each separately.

I. *Oryctognosy.*

Under this head I include the description and analysis of minerals.

1. *Native Carbonate of Magnesia.*—The carbonate of magnesia from India, of which I gave an account in the *Annals of Philosophy*, iv. 155, deserves to be mentioned, because it agrees nearly in its composition with the conite of the Germans, though it differs from it in its external characters. It is composed of

Carbonate of magnesia	72
Carbonate of lime	28
	100

This does not differ much from a compound of two integrant particles of carbonate of magnesia and one integrant particle of carbonate of lime.

2. The mineral called haüyne is hitherto scarcely known in Britain. It is an inhabitant of volcanic rocks; and has hitherto been found only in Italy, Auvergne, and at Andernach. According to Gmelin, (*Annals of Philosophy*, iv. 193,) its constituents are,

Silica	35·48
Alumina	18·87
Sulphate of lime	21·73
Lime	2·66
Oxide of iron	1·16
Potash	15·45
Water	1·20
Sulphureted hydrogen and loss	3·45
	100·00

3. My analysis of asbestous actinolite (*Annals of Philosophy*, iv. 209,) gives a great number of constituents, and on that account is not very satisfactory. I do not mean to say that the numbers given are inaccurate; for the experiments were carefully made, and most of them repeated three times; but they lead to the suspicion that the specimen was contaminated with foreign bodies; yet to the eye it was pure, and consisted entirely of a congeries of crystals. I found the mineral composed as follows:—

Silica	33·4
Alumina	28·2
Lime	1·046
Magnesia	0·6
Oxide of iron	17·15
——— manganese	7·2
Tungstic acid ?	3·84
Copper	1·0
Soda	3·8
Moisture	1·7
Loss	2·064
	100·000

4. *Green Granular Actinolite.*—This is a mineral which occurs at Teinach, in the Pacher-Alps, near Marburg, in amorphous pieces, where it constitutes a rock with grey and red quartz. It has been confounded with smaragdite or diallage; but Werner has lately separated it from this mineral. The following, according to Karsten, are the distinguishing characters of the two species:—

Green Smaragdite.	*Green Granular Actinolite.*
Lustre, pearly.	Lustre, glassy.
Fracture foliated, cleavage single.	Foliated, cleavage double.
Cross fracture uneven.	Cross fracture splintery,
Soft.	Semihard. -
Sectile.	Brittle.

Klaproth found the specific gravity of green granular actinolite 3·250. It was composed of the following constituents:—

Silica	56
Magnesia	18·5
Lime	15·5
Alumina	3·25
Oxide of iron	4·75
——— chromium	1·
——— manganese, a trace.	
Loss	1·
	100·00

5. *Schorl.*—Professor Bernhardi being of opinion that common schorl, and several other species of minerals, might be constituted into a genus, to which he proposed to give the name of tourmaline, requested Professor Bucholz to make an analysis of common schorl to serve as a sample of this new genus. Bucholz accordingly made three very careful analyses, two of schorl from St. Gotthard, and one of schorl from Tyrol. The result was as follows:—

Silica	35·000
Alumina	31·500
Magnesia	5·938
Lime	0·062
Oxide of iron	6·125
Silica containing iron	0·125
Manganese, a trace.	
Water	5·000
Potash	1·333
Loss	14·917
	100·000

This loss he was not able to account for. He could neither find any more alkali, nor any volatile matter whatever. The most probable substance seems to be fluoric acid. I do not perceive that he sought for it particularly.

6. *Arragonite.*—The discovery that arragonite contains about four per cent. of carbonate of strontian, recently made by Professor Stromeyer, is of considerable importance, by removing an anomaly which hitherto had existed in mineralogy; namely, the same chemical composition with a difference of external characters. It shows us also how much analysts ought to be on their guard, and how easy it is for the most skilful experimenters to overlook ingredients that have a marked effect upon the external characters of minerals.

6. *Schorlous Beryl.*—This mineral was analysed by Bucholz in 1804, who detected in it 17 per cent. of fluoric acid. Since that time Vauquelin (Ann. de Chim. lxvii. 247,) and Klaproth (Beitrage, v. 57,) analysed this mineral. The former found five per cent. of fluoric acid; the second, four per cent. This great discordance induced Bucholz to repeat his experiments, and his new analysis agrees very nearly with his former one. He found the constituents as follows:—

Silica	35
Alumina	48
Oxide of iron	00·5
Water	1
Fluoric acid	16·5
	101·0

7. *Prehnite.*—Two varieties of prehnite have been analysed by Gehlen: the first, from Fassuthale, had a specific gravity of 2·917; the specific gravity of the second, from Ratschinkes, was 2·924. The first variety was composed of

Silica	42·875
Alumina	21·50
Lime	26·50
Oxide of iron	3·00
——— manganese	0·25
Magnesia, a trace.	
Volatile matter	4·625
Soda	0·27
Loss	·98
	100·000

The constituents of the second variety were,

Silica	43·00
Alumina	23·25
Lime	26·00
Oxide of iron	2·00
——— manganese	0·25
Magnesia, a trace.	
Volatile matter	4·00
Loss	1·5
	100·00

Another mineral found in a dolomite rock at Hafnerszell, which from its characters and constituents he considered as a variety of prehnite, was likewise analysed by Gehlen.

This mineral is amorphous. Its colour is light greenish-grey passing into yellow. It appears foliated, or rather radiated, and exhibits a two-fold cleavage parallel to the faces of a four-sided rectangular prism. Its fracture is splintery; its specific gravity, 2·676. It gives sparks with steel, scratches glass, but is itself scratched by quartz. It is difficultly frangible. Lustre pearly, inconsiderable. Translucent. Phosphoresces when heated. Becomes strongly electric by heat. Its constituents were,

Silica	54·50
Alumina	25·25
Lime	10·05
Magnesia	3·25
Oxide of iron	1·00
——— manganese, a trace.	
Soda	5·94
	99·99

8. *Conite.*—The mineral called *conite,* described by Schumacher,

from Iceland, and by different other mineralogists, seems to me to be very nearly connected, if not quite the same, with what I have called native carbonate of magnesia from India; though its external characters, according to the following description, are not quite the same. This difference I ascribe to the absence of carbonate of iron in the Indian mineral.

Conite has a light flesh-red colour. It is amorphous, and externally covered with a coating of iron ochre. Fracture sometimes fine-grained, uneven; sometimes imperfectly conchoidal. No lustre; scratches glass; opake; brittle; sp. gr. 3·000.

Its constituents, according to the analysis of Dr. John, are as follows:—

Carbonate of magnesia	67·5
——— lime	28·0
——— iron	3·5
Water	1
Sulphate of lime? a trace.	
	100·0

9. *Zeolite.*—Haüy's division of the zeolite into stilbite and mesotype is well known; and likewise his union of the natrolite with the mesotype. Gehlen analysed two specimens of each of these genera. The following was the result:—

	Stilbite.		Mesotype.	
Silica	55·072	55·615	53·392	54·40
Alumina	16·584	16·681	19·620	19·70
Lime	7·584	8·170	1·750	1·61
Soda with some potash ..	1·500	1·536	14·696	15·09
Water	19·300	19·300	9·710	9·83
	100·040	101·302	99·168	100·63

Gehlen has rendered it probable that the mineral analysed by Vauquelin under the name of mesotype pyramidée, and even the minerals tried by Haüy before the blow-pipe, were not mesotypes, but stilbites. We see from the preceding analysis that the two minerals constitute two distinct species, well marked by the proportion of their constituents. Stilbite contains twice the water in mesotype. Mesotype contains much less lime, but much more alkali.

10. *Boracite.*—About five years ago boracite was found in abundance by Professor Steffens, in a gypsum mountain near Segeberg, in Holstein. The crystals are very small, and consist either of perfect cubes, or of cubes with their angles truncated. According to the analysis of Professor Pfaff, this boracite is composed of

Boracic acid	6
Magnesia	$3\frac{3}{8}$
Oxide of iron	$\frac{1}{16}$
Silica	$\frac{1}{4}$
Loss	$1\frac{5}{16}$
	11

Thus it is free from lime, and therefore verifies the previously received opinion that boracite is essentially a *borate of magnesia.*

11. *Vitreous Black Oxide of Iron.*—This is a mineral mentioned by Haüy in his Tableau Comparatif, p. 98 and 274, as a black vitreous substance found in the department of the Lower Rhine. It scratches glass, and has a specific gravity of 3·2. When heated to redness, it becomes magnetic. Vauquelin found it composed of oxide of iron 80·25, water 15, silica 3·75. Bucholz has lately analysed a small quantity of this mineral. He found its composition as follows:—

Oxide of iron	68·5
Silica	10·0
Oxide of manganese	5·5
Loss	16·0
	100·0

If we consider this loss as water, which seems from the analysis of Vauquelin to be the case, the mineral will be a hydrate of iron and manganese united with silica.

12. *Sulphuret of Cobalt.*—This mineral occurs in Sweden, at Nya Bastnäs, near Riddarhyttan. Hisinger found its constituents as follows:—

Cobalt	43·20
Copper	14·40
Iron	3·53
Sulphur	38·50
Earthy matter	0·33
	99·96

13. *Native Yellow Oxide of Lead.*—This rare mineral has been lately examined by Dr. John. Its characters are the following:—

External colour, between sulphur and lemon-yellow; internal, between honey-yellow and Aurora red.

Amorphous.

Fracture, earthy. In some places there is a tendency to the foliated fracture.

External lustre, dull; internal, semi-metallic.

Opake.

Semihard.

Brittle. Moderately easily frangible.

Specific gravity 8·000.

Its constituents are as follows:—

Lead	82·6923
Carbonic acid	3·8462
Oxide of iron and lime	0·4808
Copper, a trace.	
Silica mixed with iron	2·4039
Oxygen	10·5768
	100·0000

14. *Supposed Native Minium from Kall, in the Roer Department.*—I am not sure whether this be the native minium of Mr. Smithson, as I have not his dissertation at hand. Its characters are as follows:—

Colour, brownish-red. Amorphous.
Fracture fine-grained, uneven.
External lustre, dull; internal, glimmering.
Opake; soft; easily frangible; sp. gravity, 4·000.
Dr. John found its constituents as follows:—

Lead		44·15
Carbonic acid		10·00
Water		4·00
Lime and oxide of iron		0·50
Insoluble matter composed of..	Silica	29·00
	Alumina	5·25
	Oxide of iron	3·00
Oxygen		4·10
		100·00

By comparing this analysis with the preceding, it is obvious that the oxide in this mineral is not the red, but the yellow oxide of lead, and that it owes its red colour to the oxide of iron which it contains. The proportion of oxygen is obviously over-rated in both analyses.

15. *Spinell.*—A specimen of spinell, from Oker, in Sudermanland, in Sweden, analysed by Berzelius, gave the following constituents:—

Alumina	72·25
Silica	5·48
Magnesia	14·63
Oxide of iron	4·26
Water	1·83
Loss	1·55
	100·00

16. *Red Silicious Ore of Manganese.*—A specimen of this ore from Longbanshyttan, in Wermeland, in Sweden, analysed by Berzelius, was composed of

Black oxide of manganese	52·60
Silica	39·60
Oxide of iron	4·60
Lime	1·50
Water	2·75
	101·25

The excess of weight was owing to the manganese in the ore containing less oxygen than the black oxide, in which state it was obtained by analysis.

17. *Sodalite.*—Ekeberg analysed a mineral from Hefselkulla, in the province of Nerike, in Sweden, which, from his description of it, seems to have some resemblance to sodalite; but its constituents differ materially. It occurs in an iron-mine mixed with quartz. Its colour is greenish-grey; lustre, pearly and inconsiderable; principal fracture foliated with a two-fold cleavage; cross fracture granular, uneven; translucent on the edges; it scratches glass, but is scratched by steel; difficultly frangible; specific gravity 2·746. Its constituents were,

Silica	46
Alumina	28·75
Magnesia	13·50
Oxide of iron	0·75
Water	2·25
Soda	5·25
Loss	3·50
	100·00

18. *Black Garnet.*—A specimen of black garnet from the iron-mine of Svappavara, in Torneo Lappmark, analysed by Hisinger, gave the following constituents:—

Silica	34·53
Lime	24·36
Alumina	1·00
Oxide of iron	36·05
Volatile matter	0·50
Loss	3·56
	100·00

19. *Sparry Iron Ore.*—A specimen of this mineral, from Riddarhyttan, in Wermeland, in Sweden, analysed by Hisinger, was composed of

Red oxide of iron	63·25
Oxide of manganese	3·00
Lime	1·00
Carbonic acid	30·00
Water	1·75
	99·00

20. *Scapolite.*—A specimen of scapolite, from Sudermanland, in Sweden, analysed by Berzelius, was composed of

Silica	61·50
Alumina	25·75
Lime	3·00
Magnesia	0·75
Oxide of manganese	1·50
——— iron	1·50
Water	5·00
	99·00

21. *Cerite.*—This mineral, according to the last analysis of Hisinger, is composed of

Oxide of cerium	68·59
Silica	18·00
Lime	1·25
Oxide of iron	2·00
Water and carbonic acid	9·60
	99·44

22. *Spodumene.*—Spodumene, from Utön, according to the same analyst, is composed of

Silica	63·40	67·50
Alumina	29·40	27·00
Oxide of iron	3·00	3·00
Lime	0·75	0·63
Volatile matter	0·53	0·53
Loss	2·92	1·34
	100·00	100·00

The last of these analyses is by Berzelius.

23. I shall now subjoin a table of a number of Swedish minerals analysed by Hisinger:—

Lepidolite from Utön	Silica	61·60
	Alumina	20·61
	Lime	1·60
	Oxide of manganese	0·50
	——— iron, a trace.	
	Potash	9·16
	Volatile matter	1·86
	Loss	4·67
		100·00

Malacolite, from Longbanshyttan	Silica	54·18
	Lime	22·72
	Magnesia	17·81
	Oxide of iron	2·18
	——— manganese	1·45
	Volatile matter	1·20
		99·54
Serpentine, from Bojmosmine, in Norberg	Magnesia	37·24
	Silica	32·00
	Lime	10·60
	Alumina	0·50
	Oxide of iron	0·60
	Volatile matter	14·16
	Loss	4·90
		100·00
A black mineral from the iron mine of Gillinge, in Sudermanland. Sp. gr. 3·045	Oxide of iron	51·50
	Silica	27·50
	Alumina	5·50
	Oxide of manganese	0·77
	Magnesia, a trace.	
	Volatile matter	11·75
	Loss	2·98
		100·00
A violet stone from Borkhult. Sp. gr. 2·8	Silica	46·40
	Alumina	29·00
	Lime	17·14
	Oxide of iron	0·70
	Volatile matter	3·20
	Loss	3·56
		100·00
Bitter spar from Ljusnedal, in Herjeodal	Lime	29·8
	Magnesia	21·6
	Oxide of iron	1·0
	Carbonic acid	47·6
		100·0
Mealy zeolite from Fahlun	Silica	60·
	Alumina	15·60
	Lime	8·00
	Oxide of iron	1·80
	Volatile matter	11·60
	Loss	3·00
		100·00

Iridescent iron ore from Grengesberg	Red oxide of iron	94·38
	Phosphate of lime	2·75
	Magnesia	0·16
	Stoney matter	1·25
	Volatile matter	0·50
		99·04

24. *Swine-stone.*—Hisinger and Berzelius have analysed various specimens of Swedish swine-stone. The following are the results of their labours:—

Transparent swine-stone from Garphyttan, in Nerike	Carbonate of lime	99·1
	——— magnesia	0·9
	——— manganese	
	——— iron	
		100·0

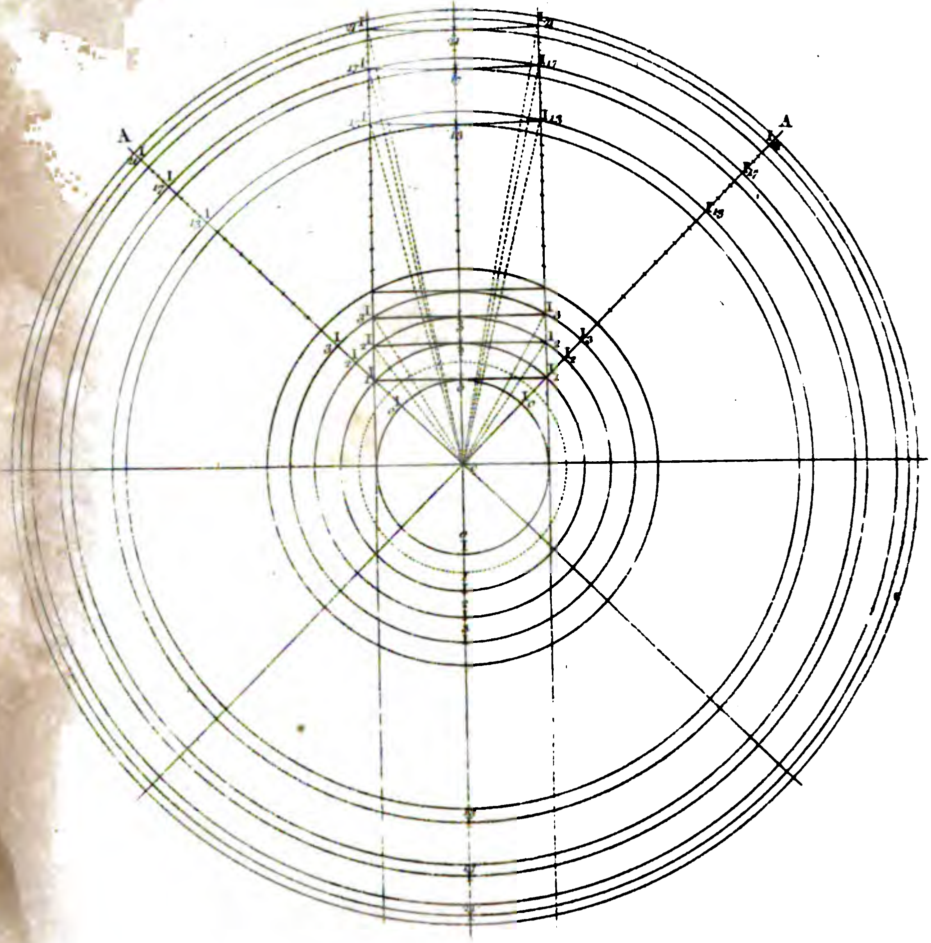
Black sparry swine-stone from the same place	Carbonate of lime	95·0
	——— manganese	1·5
	——— magnesia	
	——— iron	
	Alum-slate and iron pyrites	3·5
		100·0

Prismatic swine-stone	Carbonate of lime	98·6
	——— magnesia	0·9
	——— manganese	
	——— iron	
	Alum-slate	·05
		100·0

Prismatic swine-stone from Kinnekulle	Carbonate of lime	97·25
	——— manganese	1·25
	——— magnesia	
	——— iron	
	Alum-slate	1·50
		100·00

Gehlen has proposed two alterations in the present mode of analysing minerals; and, in his analysis of prehnite, he has shown that they may be employed with advantage. 1. He substitutes *carbonate of soda*, instead of caustic alkali, for the original fusion of the mineral in a platinum crucible. He found that this method answers even in the analysis of corundum. 2. He substitutes carbonate of barytes for nitrate of barytes when our object is to obtain the fixed alkali which we suppose to exist in any mineral. He has found that this carbonate readily acts as a flux to minerals, and that it answers better than the nitrate of the same earth.

Fig. 1.



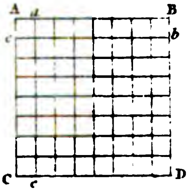


Fig. 2

London, Published by R Baldwin Paternoster Row, Jan.1.1815.

II. *Geognosy.*

I ought now to give a sketch of the recent improvements in this branch of science, which of late years has become a fashionable object of study in Great Britain; but I have already extended this article to such an enormous, and I fear improper length, that I must, however reluctantly, stop short here. The great object of geognosts on the Continent at present seems to be to trace to their utmost extent the formations discovered in the neighbourhood of Paris; and to extend as much as possible the transition formations in those countries hitherto considered as primitive. In this country we have no fewer than three geological societies, the Wernerian, the London, and the Cornwall. The first two have recently published each a volume of Transactions. I shall give an analysis of each of these books as speedily as possible. They contain almost all the important geological facts that have been lately ascertained in Great Britain.

ARTICLE II.

Solution of a Problem of Col. Silas Titus. (See Wallis's Algebra, Chap. 60.) By the Abbé Buée.*

SIR,

HAVING for many years considered the different algebraical methods for the solution of arithmetical problems by approximation to be deficient in their fundamental principles, I have been led to mistrust the whole science of algebra as generally taught, and am convinced that if we place an implicit faith in it we shall be involved in the most revolting absurdities. Pell's problem (see Wallis's Algebra, chap. 60, 62, &c.), and all those which can only be resolved by approximation, are examples of this kind. The absurdity belonging to the solution of these sort of problems is to represent numbers of which we know not the fundamental unity. In speculation this absurdity is not felt; but we easily perceive it when we quit speculation, and are engaged in questions respecting real beings, such as men. In this case the solution gives for units fractions as much smaller as the approximation is farther extended. If, then, the real unit be a man, the solution gives for unit a fraction of a man, which goes on always diminishing, and by that means becomes more and more absurd. In general the *speculative*

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* The following curious solution of a well-known problem was sent by the Abbé Buée to a mathematical gentleman in London, who declines communicating his name to the public. Though I do not participate in the Abbé's objections to algebraic approximation, yet I conceive the solution of the problem itself to be so curious as to be well entitled to the attention of mathematicians; and on that account I agreed without hesitation to insert it in the *Annals of Philosophy*. The letter which serves as an introduction to the problem is written by the Abbé Buée.—T.

unit is the QUOTIENT *of a number divided by itself,* while the *real unit is the product of a number multiplied by the inverse of that number.* Let n be any number, n divided by n is a speculative unit, and $n \times \frac{1}{n}$ is a real unit, as a rectangle would be. If, then, $\frac{1}{n}$ be the supposed unit, it is necessary, in order to obtain a real unit, to multiply $\frac{1}{n}$ by n; consequently, the nearer we approach one side, the farther we recede on the other.

The following solution, which for the first time is given of Pell's problem, is the only one exempt from this absurdity :—

Problem.

The following equations being proposed, viz.

$$a^2 + b\,c = 16 \;\ldots.\; (1)$$
$$b^2 + a\,c = 17 \;\ldots.\; (2)$$
$$c^2 + a\,b = 18 \;\ldots.\; (3)$$

To find a, b, c, (See Wallis's Algebra, chap. 60, 62, &c.) let there be a series of concentric circles, (Plate XXVII. fig. 1,) $\underset{0}{1}\;\underset{0}{1}\;\underset{0}{1}$, $\underset{1}{1}\;\underset{1}{1}\;\underset{1}{1}$, $\underset{2}{1}\;\underset{2}{1}\;\underset{2}{1} \ldots. \underset{13}{1}\;\underset{13}{1}\;\underset{13}{1} \ldots. \underset{17}{1}\;\underset{17}{1}\;\underset{17}{1} \ldots. \underset{21}{1}\;\underset{21}{1}\;\underset{21}{1}$, and let them be so described that we have $\bigcirc\,\underset{0}{1} = \underset{0}{1}\;\underset{1}{1} = \underset{1}{1}\;\underset{2}{1} = \underset{2}{1}\;\underset{3}{1} =$ &c. $= 1$.

If we make $a =$ the sector $\bigcirc\,\underset{13}{1}\;\underset{13}{1}\;\underset{13}{1}$ ...... (4)

$b =$ the sector $\bigcirc\,\underset{17}{1}\;\underset{17}{1}\;\underset{17}{1}$ ...... (5)

$c =$ the sector $\bigcirc\,\underset{21}{1}\;\underset{21}{1}\;\underset{21}{1}$ ...... (6)

Then if the areas of these sectors be substituted for a, b, c, in the equation (1), (2), (3), according to the following method, their differences will reduce these equations to identical ones.

Demonstration.

This demonstration is founded upon a remarkable property of the concentric circles of this figure. This property is, that the areas of each of the rings intercepted between two consecutive circumferences are equal to the area of the central circle. If we take the area of the central circle for an unit, the areas of each of the rings will be $= 1$.

To prove this, let $\bigcirc\,\underset{0}{1}$ be the radius of the central circle, we may easily perceive that the radii of the successive circles are the hypotheneuses of right angled triangles, whose sides are,

1. The radius of the preceding circle. 2. A constant tangent equal to the radius of the central circle. The series of radii will be then expressed by $\bigcirc\,\underset{0}{\acute{1}}\;(\sqrt{1},\ \sqrt{2},\ \sqrt{3},\ \sqrt{4},$ &c.) .... (7)

Now the circumferences of circles are constantly proportional to their radii: if, then, we designate by 2π the ratio of the circumference to its radius, the series of circumferences will be expressed by the progression $(\overline{\bigcirc \underset{0}{1}})^2 \times 2\pi\,[\sqrt{1}, \sqrt{2}, \sqrt{3}, \sqrt{4}, \text{\&c.}]$ .. (8)

But the areas of circles are as the squares of their radii; so that the series of circles will be expressed by the progression $(\bigcirc \underset{0}{1})^2 . 2\pi$ [1, 2, 3, 4, &c.] ....(9)

In this series (by taking away the common factor $(\bigcirc \underset{0}{1})^2 . 2\pi$) 1 expresses the area of the central circle; 2, 3, 4, &c. express those of the successive circles. If we take the difference of each of the areas of these consecutive circles, we shall have the areas of the rings. Now these differences are constantly equal to 1; consequently the areas of the rings are = 1.

It follows from the above conclusions that if we reckon the central circle for the first ring, the series of rings will be expressed by the common *ordinals*, 1st, 2d, 3d, 4th, &c. .... (10) while the series of circles are expressed by the *absolute* numbers 1, 2, 3, 4, &c. ....(11)

These ordinal numbers follow the *direct* or *inverse* order: when they follow the *direct* order, the areas of the rings are *positive*; when they follow the *inverse* order, they are *negative*. These areas are constantly = 1, and are represented by the equation $+1 = e^{\pm 2k\pi\sqrt{-1}}$ .... (12)

(k being any positive whole number.)

And the negative areas by $-1 = e^{\pm (2k+1)\pi\sqrt{-1}}$ ....(13)

These equations, which are fundamental, I thus demonstrate: in every system of logarithms the logs. are exponents, and *these exponents are ordinal numbers*, because they are the indexes of the terms of a geometrical progression whose first term is 1. Now from the principles demonstrated by Euler (Introductio in Analysin Infinitorum, Cap. VIII. No. 39,) we may prove the truth of the two following equations:—

$$\text{Log.}\,(+1) = \pm 2k\pi\sqrt{-1} \qquad (14)$$

$$\text{Log.}\,(-1) = \pm 2(k+1)\pi\sqrt{-1} \qquad (15)$$

(See Lacaille's Leçons de Mathematiques, Nos. 833, 834, 835.)

Let e be the base of the hyperbolic logarithm: we have $1 = \log. e$ ....(16)

Consequently $\log.\,(+1) = \pm 2k\pi\sqrt{-1}\,\log. e$ ......(17)

and $\log.\,(-1) = \pm 2(k+1)\pi\sqrt{-1}\,\log. e$ ....(18)

From the above we easily derive the equations (12), (13). The following is an explanation of these equations: k can only be an ordinal number, because it is the only variable factor of the exponent $\pm 2 k \pi \sqrt{-1}$. Thus $2 k \pi$ represents *that* circumference whose place is designated by k.

$\pm \sqrt{-1}$ is a sign of *impossibility*, because it expresses a quantity greater than a *maximum*, and less than a *minimum*; but the area of each concentric ring is out of that circle which serves as a nucleus, and it is the diameter of *that* circle which is a *maximum.* The diameter, then, of the exterior circle of this ring is greater than a *maximum.* The whole area of the ring which exceeds this diameter is then proved imaginary, which shows that the sign $\pm \sqrt{-1}$ belongs absolutely to it. It now remains to explain the sign ±. A ring contains two circumferences of circles; to wit, an external and an internal. Now 2π only expresses one; the sign ± causes it to express two; which I thus prove:—

Let $y y = a a - x x$ ....(19)

be the equation to a circle: if we take the value of y we shall have

$y = \sqrt{a a - x x}$ ....(20)

Here the double sign indicates two *ordinates of an equal length drawn from any particular point of the diameter on each side of it.* The positive ordinates, designated by +, extend only to half the circle; and the negative ordinates, designated by −, extend to the other half: in order to obtain the ordinates which extend to the whole circle, we must unite the two signs, as in ±. Now when this sign is accompanied by $\sqrt{-1}$, it does not mean + *or* −, but + *and* −; because the imaginary quantities always go in pairs, and they cannot be separated without an absurdity, as I will prove.

Thus let $1_1 1_0 1_1$ be the tangent to the central circle. This tangent is the *greatest* ordinate which can be drawn to the exterior circle *without entering into the central circle* $1_1 1_1 1_1$: its middle point is at the same time the *smallest* of those which can be drawn in the interior circle, since it is reduced to this point, 1_0, in which the two ordinates coincide, the two ordinates, $1_1 1_0$ and $1_0 1_1$ having then the point 1_0, which is common to both, and are connected by that point. Thus they form a continued right line, which is expressed by $\pm \sqrt{-1}$. If we refer this expression to the interior circumference, we have $\pm \sqrt{-1} = 0$, which is not imaginary, because then it is the sign $\sqrt{-1}$, which ought to be considered as 0. If, on the contrary, we refer it to the extreme circumference, we have $\pm \sqrt{-1} = +\sqrt{-1} - \sqrt{-1}$, which only ceases to be imaginary at the two points $1_1 1_1$, which coincide with this extreme circumference. To apply this principle to the double sine of the expression $\pm 2\,k\pi\sqrt{-1}$, let us divide into two equal parts the part $\underset{0}{1}\,\underset{1}{1}$ of the diameter $\underset{1}{1}\,\overset{1}{1}$, which is intersected between the two circumferences of the ring which extends beyond the central circle; and through the point of division let us draw the dotted concentric circle; the circumference of the dotted circle will be an *arithmetical mean proportional* between the two extreme circumferences of the ring. If we take this dotted circumference as a line of abscissa, it is clear it will cut all the sections of the diameters intercepted between the extreme circumference into two equal parts. Each of these half parts will be equal ordinates drawn on each side of the circumference of the dotted circle, this circumference being taken as a *line of abscissa*, and the two extreme circumferences will be the curves described by means of these ordinates. As all these ordinates are imaginary, they have only two real points, which are their two extremes: one of these two extremes is a point in the dotted circle, and the other is a point in one of the circles already described: these three circles are then composed only of insulated points, the points of the dotted circle are double, and those of the circle described are simple. $\pm 2\,\pi\sqrt{-1}$ expresses the sum of the points of the dotted circle; that is to say, $+ 2\,\pi\sqrt{-1}$ is the sum of the points of the exterior circle, and $- 2\,\pi\sqrt{-1}$, the sum of the points of the interior circle. Resuming all this explanation, we find $\pm 2\,k\pi\sqrt{-1}$ *is the sign of the description of two concentric circles forming a ring by assuming for a line of abscissa a third concentric circle whose circumference is an arithmetical proportional mean between the circumferences to be described*, the same as $\pm\sqrt{aa - xx}$ is the sign of *the description* of a simple circumference by taking its diameter for a line of the abscissa.

This granted, in order to resolve the equations (1), (2), (3), I begin by multiplying their second member by the second member of equation (12), which gives me

$a^2 + b\,c = 16\,e^{\pm 2\pi\sqrt{-1}}$ = the area* of the 16th. circle, (fig. 1) ....(22)

$b^2 + a\,c = 17\,e^{\pm 2\pi\sqrt{-1}}$ = the area of the 17th circle..(23)

$c^2 + a\,b = 18\,e^{\pm 2\pi\sqrt{-1}}$ = the area of the 18th circle..(24)

I assume for the roots of these equations,

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* The idea of my giving one area for the root of another area, may perhaps be cavilled at; but when we consider that the root of the area of the square A B C D (fig. 2) can only be the area of a rectangle, such as A *a* C c or A c B *b*, the explanation will appear clear.

$a = e^{\pm \frac{13}{4} \cdot 2\pi \sqrt{-1}}$ = area of the 13th quadrant........(25)

$b = e^{\pm \frac{17}{4} \cdot 2\pi \sqrt{-1}}$ = area of the 17th quadrant ....(26)

$c = e^{\pm \frac{21}{4} \cdot 2\pi \sqrt{-1}}$ = area of the 21st quadrant ....(27)

To prove that these are the *true roots* of equations (22), (23), (24), I substitute these roots for *a*, *b*, *c*; then take the differential*, considering the sign ± as the differential sign. These substitutions give me

$$e^{\pm \frac{26}{4} \cdot 2\pi \sqrt{-1}} + e^{\pm \left(\frac{17+21}{4}\right) \cdot 2\pi \sqrt{-1}} = 16 e^{\pm 2\pi \sqrt{-1}} \quad (28)$$

$$e^{\pm \frac{34}{4} \cdot 2\pi \sqrt{-1}} + e^{\pm \left(\frac{13+21}{4}\right) \cdot 2\pi \sqrt{-1}} = 17 e^{\pm 2\pi \sqrt{-1}} \quad (29)$$

$$e^{\pm \frac{42}{4} \cdot 2\pi \sqrt{-1}} + e^{\pm \left(\frac{13+17}{4}\right) \cdot 2\pi \sqrt{-1}} = 18 e^{\pm 2\pi \sqrt{-1}} \quad (30)$$

By taking the differential, I obtain

$\pm \left(\frac{26}{4} \cdot 2\pi \sqrt{-1}\right) e^{\pm \frac{26}{4} \cdot 2\pi \sqrt{-1}} \pm \left(\frac{17+21}{4} 2\pi \sqrt{-1}\right)$
$e^{\pm \left(\frac{17+21}{4}\right) 2\pi \sqrt{-1}} = \pm 2\pi \sqrt{-1} \times 16 e^{\pm 2\pi \sqrt{-1}} =$
the area of the 16th ring ....(31)

$\pm \left(\frac{34}{4} \cdot 2\pi \sqrt{-1}\right) e^{\pm \frac{34}{4} \cdot 2\pi \sqrt{-1}} \pm \left(\frac{13+21}{4} 2\pi \sqrt{-1}\right)$
$e^{\pm \left(\frac{13+21}{4}\right) 2\pi \sqrt{-1}} = \pm 2\pi \sqrt{-1} \times 17 e^{\pm 2\pi \sqrt{-1}} =$
area of the 17th ring ....(32)

$\pm \left(\frac{42}{4} \cdot 2\pi \sqrt{-1}\right) e^{\pm \frac{42}{4} \cdot 2\pi \sqrt{-1}} \pm \left(\frac{13+17}{4} 2\pi \sqrt{-1}\right)$
$e^{\pm \left(\frac{13+17}{4}\right) 2\pi \sqrt{-1}} = \pm 2\pi \sqrt{-1} \times 18 e^{\pm 2\pi \sqrt{-1}} =$
area of the 18th ring ....(33)

The experimental quantities of equation (31) are reducible to

$$\left.\begin{cases} e^{\pm 13\pi \sqrt{-1}} = -1 \\ e^{\pm 19\pi \sqrt{-1}} = -1 \end{cases}\right\} \text{(Vid. equation 13)} \begin{cases} \ldots\ldots\ldots (34) \\ \ldots\ldots\ldots (35) \end{cases}$$

$$\begin{cases} e^{\pm 17\pi \sqrt{-1}} = -1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (36) \\ e^{\pm 17\pi \sqrt{-1}} = -1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (37) \end{cases}$$

The equation (33) becomes

$e^{\pm 21\pi \sqrt{-1}} = -1$ ....(38)

$e^{\pm 15\pi \sqrt{-1}} = -1$ ....(39)

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* This kind of differential is the true and strict meaning of Lemma II. Sect. II. Book II. of Newton's Principia. (Momentum Genitæ, &c.) The manner in which Newton has demonstrated this lemma entirely refutes every possible objection.

By substituting these values of the exponential in the equations (31), (32), (33), they take the following forms :—

$$\mp \tfrac{26}{4} \cdot 2\pi\sqrt{-1} \mp \tfrac{38}{4} \cdot 2\pi\sqrt{-1} = \pm 2\pi\sqrt{-1} \times 16 \,..(40)$$

$$\mp \tfrac{34}{4} \cdot 2\pi\sqrt{-1} \mp \tfrac{34}{4} \cdot 2\pi\sqrt{-1} = \pm 2\pi\sqrt{-1} \times 17 \,..(41)$$

$$\mp \tfrac{42}{4} \cdot 2\pi\sqrt{-1} \mp \tfrac{30}{4} \cdot 2\pi\sqrt{-1} = \pm 2\pi\sqrt{-1} \times 18 \,..(42)$$

Now 2π expresses the circumference, of which one half is on the positive side, and the other half on the negative side: in respect to the same diameter, -2π expresses the same things taken in contrary directions; consequently $\pm 2\pi$ and $\mp 2\pi$ are two different expressions of the *same* circumference, when the diameters are the same; they then represent the *same* rings; and the last three equations give by reduction the following:—

$$\pm 2\pi\sqrt{-1}\left(\frac{26 \times 38}{4}\right) = \pm 2\pi\sqrt{-1} \times \tfrac{64}{4} = \pm 2\pi\sqrt{-1} \times 16 \,\ldots\ldots\ldots\ldots(43)$$

$$\pm 2\pi\sqrt{-1}\left(\frac{34 \times 34}{4}\right) = \pm 2\pi\sqrt{-1} \times \tfrac{68}{4} = \pm 2\pi\sqrt{-1} \times 17 \,\ldots\ldots\ldots\ldots(44)$$

$$\pm 2\pi\sqrt{-1}\left(\frac{42 + 30}{4}\right) = \pm 2\pi\sqrt{-1} \times \tfrac{72}{4} = \pm 2\pi\sqrt{-1} \times 18 \,\ldots\ldots\ldots\ldots(45)$$

These last equations are evidently identical, and the problem proposed is now completely resolved by the roots (25), (26), (27), which, expressed arithmetically, are

$$a = \tfrac{13}{4} = 3\tfrac{1}{4} \,\ldots\ldots\ldots\ldots(46)$$

$$b = \tfrac{17}{4} = 4\tfrac{1}{4} \,\ldots\ldots\ldots\ldots(47)$$

$$c = \tfrac{21}{4} = 5\tfrac{1}{4} \,\ldots\ldots\ldots\ldots(48)$$

Unity being the area of any circle, which is an essential remark; for if *unity* were an abstract number, this solution would be absurd. It is only true when unity is expressed by $e^{\pm 2\pi\sqrt{-1}}$ (= the area of any ring or central circle.)

Remark.

Wallis, who devoted much time and attention to the proposed problem, resolved the equations (1), (2), (3), by approximation. (Wallis's Algebra, Chap. 62.) His roots are,

$$a = 2,525,513,986,744,158 \,\ldots\ldots\ldots\ldots(49)$$

$$b = 2,969,152,768,619,848 \,\ldots\ldots\ldots\ldots(50)$$

$$c = 3,240,580,681,617,174 \,\ldots\ldots\ldots\ldots(51)$$

Comparing these roots with the roots (46), (47), (48), it is easily perceived that they have not the least relation to each other. In order to prove how nearly the roots (49), (50), (51), verify the equations (1), (2), (3), Wallis has substituted them in the place of a, b, c, by which he obtained the following results;—

$$16 = a^2 + bc = 16{,}000{,}000{,}000{,}000{,}000 \ldots\ldots. (52)$$
$$17 = b^2 + ac = 17{,}000{,}000{,}000{,}000{,}000 \ldots\ldots. (53)$$
$$18 = c^2 + ab \doteq 17{,}999{,}999{,}999{,}999{,}997 \ldots\ldots. (54)$$

However near Wallis's approximation may be, the equations which he obtained are not for that reason the less absurd, nor are they the less

Equations, whose two members are not equal and never can become so.

In order that the two members should be equal, it is necessary that the inequality of the units compensate the inequality of the numbers, as in the equation 2*l.* = 40*s.*; this compensation takes place if each member and its unit are in an inverse ratio to each other. This is the case in my solution, where *the height of each ring, multiplied by the mean circumference between the two extreme circumferences, give for the product the constant area of the central circle*, as I thus demonstrate, for we have for each ring the following proportion:

++ *The sum of the radii of the extreme circumferences of the rings*
: *To the tangent drawn to the smallest of these circumferences*
:: *This tangent*, (which is = the radius of the central circle,)
: *The difference of the radii of the extreme circumferences of the rings.* (This difference being the height of the ring, 185.)

From whence we have the following theorem.

In a series of concentric rings, each of whose areas are equal to the central circle. The rectangle formed by the sum of the radii of the extreme circumferences of any one of the rings and its height, is equal to the square of the central circles. (56.) Now the area of each ring is equal the area of a trapezium, which has for its base the height of the ring, and for the mean height half the sum of the extreme circumferences. But this half is equal to a mean circumference between the two extreme circumferences. We can then transform theorem (56) into the following.

In a series of concentric rings whose areas are equal to the area of the central circle, the rectangle formed of the height of each ring and the circumference, which is a mean between the two extreme circumferences, is equal the area of the central circle, which is the proposition I had to demonstrate.

General Corollary.

The preceding resolution of equations (1), (2), (3), gives a complete solution of Gauss's problem, viz. "*To divide a circumference into* 17 *equal parts.*" The dotted isosceles triangle $O\ 1_{17}\ 1_{16}\ 1_{17}$ (fig. 1) whose summit is the centre O, has for its base the continued line $1_{17}\ 1_{16}\ 1_{17}$; it is this base whose extremities are $1_{17}\ 1_{17}$, which divides the circumference into 17 parts. The arc $1_{17}\,1_{17}\,1_{17}$ is one of these parts; the continued line $1_{17}\,1_{16}\,1_{17}$ is equal to the central diameter $1_{0}\bigcirc 1^{0}$. Now this central diameter is a unit; for the central circle and its diameter are respectively units, which are heterogeneous with each other. All the parallel lines, $1_{1}\,1_{0}\,1_{1}$, $1_{2}\,1_{1}\,1_{2}$, &c. are equal to this last unit. These lines are the greatest which can be drawn between the two extreme circumferences of each ring, and they may be considered as the diagonals of those rings, in the same manner as the diameter $1_{0}\bigcirc 1^{0}$ can be considered as the diagonal of the central circle. We have, then, between the diagonal of the central circle and the diagonal of the rings, the same analogy as between the central circle itself and these rings; the central circle and the rings *are* both superficial units, and *maxima*; the diagonals of the central circle and of the rings *are* both *linear* units, and also *maxima*. This geometrical construction of the problem, as presented by fig. 1, is the only one which can be right; because all the straight lines of this figure are either *maxima* or *minima*, in such a manner that if they be maxima as lines, they are *minima* as being opposite to an angle or an arch which is a *minimum*; and if they be *minima* as lines, they are *maxima*, as being opposite to an angle or an arch which is a *maximum*, as I shall demonstrate at length in another paper. These lines, then, cannot be greater or less than they are. M. le Gendre, at the end of his Geometry, has given an algebraical solution of this problem, but he has not given the geometrical construction of his formulæ: that construction was in fact impossible, without destroying the law of continuity.

ARTICLE III.

*Some Experiments on pure Nickel, its Magnetic Quality, and its Deportment when united to other Bodies.** By W. A. Lampadius.

1. AFTER having in 1796 discovered a method of obtaining pure malleable nickel by means of an oxygen gas fire, either from Freiberg *Bleispeise,* or from the common regulus of nickel obtained from copper nickel by the usual process, I occasionally made a number of accurate experiments on many of the properties of this metal, which had been hitherto examined only in a cursory manner. 124 grains of *speise* gave me 43 grains of nickel, and 123 grains of copper nickel ore gave me 63 grains of the pure metal.

2. *Magnetic Power of Nickel.*

The magnetometer described in the preceding paper gave the

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* Translated from Schweigger's Journal für Chemie und Physik, x. 174. 1814.

magnetic energy of nickel = 35, and that of iron = 55. The magnetic energy of cobalt was likewise tried, and found = 25; but as this metal was not quite pure, this experiment, as well as the magnetism of an alloy of cobalt and nickel, will be hereafter repeated.

3. *Alloy of Nickel and Platinum.*

This, as well as all the subsequent alloys, was made upon charcoal kept intensely hot by a stream of oxygen gas, according to the method described in my Manual for the Analysis of Minerals. A grain of each of the two bodies, nickel and platinum, was put upon the charcoal. After they had been softened by the application of the heat for about half a minute, both bodies incorporated together in a very striking manner. They formed an alloy possessing nearly the fusibility of copper, although nickel by itself is nearly as infusible as platinum. The alloy was completely malleable, acquired a fine polish, and had a light yellowish-white colour, not unlike that of sterling silver. Its magnetic energy was still 35.

4. *Gold and Nickel (equal parts).*

Both metals very readily melt into one round button; pretty hard, harder than the preceding alloy; externally malleable; capable of a fine polish; colour yellowish-white, a little darker than the preceding alloy. The magnetism continued = 35.

5. *Silver and Nickel (equal parts).*

When I attempted to alloy these two metals, I made the following observations. The silver melted in two seconds, and the nickel remained for some time unmelted upon the silver. In about a minute the silver, it is true, took up the nickel, but did not dissolve it. When the heat was continued some time longer, the two metals appeared to unite; but at that instant the silver burnt away with a blue flame, and left the malleable nickel behind it; but about one half of the nickel likewise was burnt.

6. *Copper and Nickel (equal parts).*

Both metals melted together in four seconds. The alloy was brittle and granular; the colour reddish-white; and the fracture porous. It exhibited no trace of magnetism.

7. *Nickel and Iron.*

Iron and nickel easily melted together into a round bead. The nickel was first melted, and the iron added to it, to prevent the last metal from being burnt by the heat. By continuing the heat, the greater part of the iron separated from the nickel in the state of a black oxide, still attracted by the magnet. By weighing the alloy I found that it consisted of ten parts of nickel and four parts of iron, or the iron amounted to rather less than one-third of the alloy. This alloy was moderately hard, quite malleable, and had the colour of steel. Its magnetism was = 35.

8. *Phosphorus and Nickel.*

The bead of nickel was heated red-hot, and then a small piece of phosphorus placed in contact with it. They melted together in a few seconds. 34 parts of nickel thus treated increased in weight five parts; so that 100 nickel had combined with 15 phosphorus. The button externally was tin-white, and had the metallic lustre. It was moderately hard, and very brittle. Its fracture was foliated and crystalline, partly dull, and partly with the metallic lustre. Its magnetism was gone.

9. *Nickel and Sulphur*

Easily united together, when treated in the same way as the nickel and phosphorus had been. Externally the button was dull, swelled, and grey in colour. Its magnetism likewise was gone. 20 parts of nickel had taken up two parts of sulphur; so that 100 of the metal combine with 10. The mass was elastic, not very hard, the fracture uneven, and the colour yellowish-white, similar to that of native copper nickel ore.

10. From these experiments we learn,

a. The readiness with which nickel and platinum unite together.

b. The little affinity between silver and nickel, as the silver rather combines with oxygen and is dissipated, than remains united to the nickel.

c. The singular effect of combining it with copper, in which we see two malleable metals produce a brittle alloy.

d. The permanence of the magnetism of nickel when it is alloyed with gold and platinum.

e. Its complete destruction when nickel is alloyed with copper.

f. Its diminution when nickel is alloyed with iron.

Perhaps a farther prosecution of these experiments might have a tendency to throw some light upon magnetism. At present I lay aside all hypotheses, and satisfy myself with stating simple facts.

ARTICLE IV.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North. Longitude West in Time $6''\frac{89}{100}$.

1814.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Nov. 18	—h	—′	—°	—′	—″	1h	40′	24°	19′	59″	Not observed.	Not observed.
Ditto 19	8	35	24	16	12	1	45	24	21	22		
Ditto 20	8	45	24	15	40	1	35	24	20	05		
Ditto 21	8	40	24	17	47	1	35	24	20	22		
Ditto 22	8	45	24	18	15	1	35	24	21	17		
Ditto 23	8	40	24	17	22	1	40	24	20	48		
Ditto 25	—	—	—	—	—	1	35	24	26	10		
Ditto 26	8	45	24	17	04	1	45	24	20	04		
Ditto 27	8	45	24	16	03	1	45	24	20	33		
Ditto 28	8	40	24	17	41	1	30	24	21	29		
Ditto 29	8	45	24	17	34	1	25	24	19	30		
Ditto 30	8	45	24	17	43	—	—	—	—	—		

1814.

Mean of Observations in Nov.	Morning	at	8h	41′	Variation	24°	16′	20″	West.
	Noon	at	1	40	Ditto	24	20	37	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	39	Ditto	24	14	08	West.
	Noon	at	1	42	Ditto	24	21	45	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	32	Ditto	24	14	33	West.
	Noon	at	1	39	Ditto	24	23	17	
	Evening	at	6	19	Ditto	24	16	50	
Ditto in Aug.	Morning	at	8	30	Ditto	24	14	13	West.
	Noon	at	1	39	Ditto	24	23	48	
	Evening	at	6	57	Ditto	24	16	31	
Ditto in July.	Morning	at	8	41	Ditto	24	13	29	West.
	Noon	at	1	42	Ditto	24	23	44	
	Evening	at	6	58	Ditto	24	17	00	
Ditto in June.	Morning	at	8	44	Ditto	24	13	10	West.
	Noon	at	1	30	Ditto	24	22	48	
	Evening	at	6	52	Ditto	24	16	29	
Ditto in May.	Morning	at	8	45	Ditto	24	13	12	West.
	Noon	at	1	44	Ditto	24	22	13	
	Evening	at	6	38	Ditto	24	16	14	
Ditto in April.	Morning	at	8	45	Ditto	24	12	53	West.
	Noon	at	1	48	Ditto	24	23	53	
	Evening	at	6	29	Ditto	24	15	30	
Ditto in March.	Morning	at	8	52	Ditto	24	14	29	West.
	Noon	at	1	52	Ditto	24	23	08	
	Evening	at	6	11	Ditto	24	15	33	
Ditto in Feb.	Morning	at	8	47	Ditto	24	14	50	West.
	Noon	at	1	52	Ditto	24	20	58	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	52	Ditto	24	15	05	West.
	Noon	at	1	53	Ditto	24	19	03	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at	8	53	Ditto	24	17	39	West.
	Noon	at	1	51	Ditto	24	20	30	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	40	Ditto	24	17	17	West.
	Noon	at	1	54	Ditto	24	20	24	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	Ditto	24	15	41	West.
	Noon	at	1	59	Ditto	24	22	53	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	Ditto	24	15	46	West.
	Noon	at	2	02	Ditto	24	22	32	
	Evening	at	6	03	Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	Ditto	24	15	55	West.
	Noon	at	2	02	Ditto	24	23	32	
	Evening	at	7	05	Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	Ditto	24	14	32	West.
	Noon	at	1	50	Ditto	24	23	04	
	Evening	at	7	08	Ditto	24	16	43	
Ditto in June.	Morning	at	8	30	Ditto	24	12	55	West.
	Noon	at	1	33	Ditto	24	22	17	
	Evening	at	7	04	Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	Ditto	24	12	02	West.
	Noon	at	1	37	Ditto	24	20	54	
	Evening	at	6	40	Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	Ditto	24	09	18	West.
	Noon	at	0	59	Ditto	24	21	12	
	Evening	at	5	46	Ditto	24	15	25	

Magnetical Observations continued.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Dec. 1	8h 45′	24° 18′ 52″	—h —′	—° —′ —″	Not observed.	Not observed.
Ditto 2	8 45	24 16 53	1 30	24 20 04		
Ditto 3	8 45	24 17 04	1 00	24 21 07		
Ditto 4	— —	— — —	1 40	24 20 01		
Ditto 5	8 45	24 17 28	— —	— — —		
Ditto 6	8 40	24 17 18	1 45	24 21 18		
Ditto 7	8 45	24 17 10	1 35	24 20 30		
Ditto 8	8 45	24 17 36	— —	— — —		
Ditto 9	8 35	24 17 19	1 25	24 19 35		
Ditto 10	8 35	24 18 00	— —	— — —		
Ditto 11	— —	— — —	1 20	24 19 59		
Ditto 12	8 45	24 18 12	1 35	24 18 55		
Ditto 13	8 45	24 20 20	1 30	24 20 48		
Ditto 14	8 50	24 20 14	1 40	24 21 44		
Ditto 15	8 45	24 19 40	— —	— — —		
Ditto 16	8 55	24 18 14	1 55	24 19 40		
Ditto 17	8 50	24 19 46	1 30	24 21 19		

Comparison of the Variations in the Years 1813 *and* 1814.

		1813.	1814.	Difference.
April	Morning	24° 09′ 18″	24° 12′ 53″	+ 3′ 35″
	Noon	24 21 12	24 23 53	+ 2 41
	Evening	24 15 25	24 15 30	+ 0 05
May	Morning	24 12 02	24 12 49	+ 0 47
	Noon	24 20 54	24 22 13	+ 1 19
	Evening	24 13 47	24 16 14	+ 2 27
June	Morning	24 12 35	24 13 10	+ 0 35
	Noon	24 22 17	24 22 48	+ 0 31
	Evening	24 16 04	24 16 18	+ 0 44
July	Morning	24 14 32	24 13 29	− 1 03
	Noon	24 23 04	24 23 44	+ 0 40
	Evening	24 16 43	24 17 00	+ 0 17
Aug.	Morning	24 15 55	24 14 13	− 1 42
	Noon	24 23 32	24 23 48	+ 0 16
	Evening	24 16 08	24 16 31	+ 0 23
Sept.	Morning	24 15 46	24 14 33	− 1 16
	Noon	24 22 32	24 23 17	+ 0 45
	Evening	24 16 04	24 16 50	+ 0 46
Oct.	Morning	24 15 41	24 14 08	− 1 33
	Noon	24 22 53	24 21 45	− 1 08
	Evening	— — —	— — —	— —
Nov.	Morning	24 17 17	24 16 20	— 0 57
	Noon	24 20 24	24 20 37	+ 0 13
	Evening	— — —	— — —	— —

In taking the mean of the observations for the month of November, the variation on the 4th and 25th are rejected, as being unusually great, for which no cause can be assigned.

Dec. 9.—The needle vibrated at intervals 26′ 20″; and in the evening the wind blew very hard and squally from the S.W. accompanied by rain.

Dec. 16.—The needle vibrated 15′; wind high and squally from the S.W., followed by rain.

Rain fallen { Between noon of the 1st Nov. / Between noon of the 1st Dec. } 2·207 inches.

Article V.

On the Conversion of Cast-Iron Pipes into a Substance bearing some resemblance to Plumbago. By William Henry, M.D. F.R.S. &c.

I was lately requested by a gentleman who resides in the neighbourhood of Newcastle-upon-Tyne, to examine the nature of the change effected in a cast-iron pipe, placed in the shaft of a coal mine near that town. In sinking the shaft, it was necessary, as sometimes happens, to put down a *curb*, or cylinder, of cast-iron, in order to support a bed of quicksand; and into a suitable opening in this cylinder, the cast-iron pipe, three inches diameter, was bolted by means of a *flanche* at its extremity. Its use was to allow an exit to the water and gas, which issued from the stratum of quicksand.

The fragment of the pipe, with which I was furnished, was of a dark grey colour; its inner surface was smooth and black; and its outer surface had a thin ochrey incrustation. The usual fracture of cast-iron was exchanged for an earthy one, except near the centre of the mass, where somewhat of the usual texture of cast iron was still visible. It was soft enough to be easily scraped by a knife; and was readily broken by a slight blow of a hammer. Some parts of it left a black trace on writing paper, but destitute of the lustre which the traces of plumbago exhibit.

The specific gravity of the specimen was 2·008; and, after being soaked an hour or two in water, it became 2·155.

Twenty grains of the powdered substance were projected upon 200 grains of melted nitre. The combustion was very feeble, compared with that which happens to plumbago under similar circumstances. After washing off the nitre, there remained 14½ grains, which were almost entirely oxide of iron, consisting probably of 11·2 iron and 3·3 oxygen.

A small piece of the pipe, weighing 6½ grains, was passed into a glass tube containing diluted sulphuric acid over mercury. The acid acted very slowly; and, in eight days, when its effect seemed to be complete, only half a cubic inch of hydrogen gas was formed. This was only about $\frac{1}{26}$th of what would have been obtained from the same quantity of cast-iron.* There remained a black and bulky substance, which, when dried and projected on red-hot nitre, burnt rather more vividly than the original substance, but still afforded much oxide of iron.

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* A piece of plumbago from Borrowdale, kept eight days in dilute sulphuric acid, had evolved no gas.

It was clear, therefore, that a large share of the metallic part of the pipe had been removed; and that what remained was composed of iron, plumbago, and other impurities usually present in cast-iron. With the view of ascertaining the cause of the change, I next examined the water from the bed of quicksand, a bottle of which had been sent along with the fragment of pipe.

The water had a brackish taste, and was of the specific gravity 1008. It gave no traces of iron, either with triple prussiate of potash, or succinate of ammonia. I collected the gas from a wine pint of it; but it was lost by accident before being examined. By the usual methods of analysis, a wine pint gave 64 grains of dry salt, composed of

Muriate of soda	32
——— magnesia	16
——— lime	10
Sulphate of lime	4
Carbonate of lime	2
	64

It is most probably to the agency of the muriates of lime and magnesia, that we are to ascribe the removal of the metallic part of the pipe. I have often remarked the effect of solutions of these salts in discharging writing ink from the labels of bottles, to which they have been accidentally applied: and I was lately baffled in several attempts to restore the legibility of some of the M.S.S. of a most accomplished scholar (the late Mr. Tweddell of Trin. Coll. Cambridge) which had lain some time under sea water, abounding, as is well known, in muriate of magnesia. The texture of the paper was entire, but the iron basis of the ink, as well as the gallic acid, was entirely removed.

In that copious repository of valuable knowledge, Dr. Priestley's Experiments and Observations on Air,* some facts are stated, that bear an analogy to the one which I have described. Cast-iron nails, he found, dissolved very slowly in diluted sulphuric acid; and left a large proportion of black matter, which had the original form of the nails. This experiment, he observes, explains what happens to cast-iron pipes in pits, the water of which is impregnated with vitriolic acid; for, in time, they become quite soft, or, as it is called, *rotten*, and may be cut with a knife.

In Cornwall, I am informed, cast-iron pipes are disused in many of the mines; but this is owing to the presence of sulphate of copper in the water, the corroding effects of which render it necessary to substitute pipes of brass or copper.

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* Abridged and methodized, vol. iii. part iii. sect. 2.

The following fact, which was lately observed at the printing-works of a friend of mine, belongs to a different class of phenomena; and is perhaps to be accounted for by galvanic agency. In order to confine the heat in some cast-iron steam-pipes, they were placed in a trough or gutter made of bricks, into which powdered charcoal was tightly rammed. At a place contiguous to a joint, formed by bolting two flanches together, a leak had happened; and when the iron pipe was taken up, it was found in the neighbourhood of the leaky part, to be perfectly soft and rotten. I was not able to obtain an opportunity of examining the nature of the change by any experiments on the altered iron.

Manchester, Dec. 1814.

Article VI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday the 24th of November, part of a paper by Dr. Brewster was read, on the depolarisation of light by mineral, vegetable, and animal bodies.

On Wednesday the 30th of November, the day of the anniversary election, the Copleyan medal was given to Mr. Ivory for his Mathematical Papers published in the Transactions. The following office-bearers were elected for the ensuing year.

President.—Right Hon. Sir Joseph Banks, Bart. K. B.
Secretaries.—William Hyde Wollaston, M. D.
Taylor Combe, Esq. M. A.
Treasurer.—Samuel Lysons, Esq.

OF THE OLD COUNCIL.

Right Hon. Sir Joseph Banks.
Sir Charles Blagden, Knt.
Samuel Goodenough, Lord Bishop of Carlisle, V. P.
Taylor Combe, Esq. Secretary.
Samuel Lysons, Esq. Treasurer, V. P.
George, Earl of Morton, K. T. V. P.
Thomas Murdoch, Esq.
John Pond, Esq. Astr. Royal.
William Charles Wells, M. D.
William Hyde Wollaston, M. D. Secretary.
Thomas Young, M. D. Seccretary for Correspondence.

OF THE NEW COUNCIL.

Mr. William Allen.
William Blake, Esq.

Rev. Charles Burney, LL. D.
Charles William, Earl of Charleville.
Davies Giddy, Esq. M. P.
Sir Everard Home, Bart.
James Horsburgh, Esq.
Alexander Marcet, M. D.
Thomas, Earl of Selkirk.
Henry Warburton, Esq.

The number of deaths since the last anniversary election has been 16; the number of new members admitted, 23. The number of ordinary members on the election list amounts to 580, that of foreign members to 43, making a total of 623 members.

On Thursday Dec. 8, Dr. Brewster's paper on the depolarisation of light by different bodies was continued. He gave a long list of animal, vegetable, and mineral bodies, that have the property of depolarising light; together with a list of bodies that have no effect in producing this depolarisation. He then gave his theory of this depolarisation. Bodies capable of depolarising light may be divided into seven classes. 1. Those that have a neutral axis and produce a double image. With respect to these bodies the theory is evident. 2. Those that have a neutral axis but produce only a single image, as the human hair. In these bodies he supposes that two images are really produced, but that they coincide with each other.

On Thursday Dec. 15, Dr. Brewster's paper was concluded. He continued his enumeration of the different classes of depolarising bodies as follows:

3. Those that have no depolarising axis but depolarise light in every direction, as gum arabic. These he conceives to be composed of layers placed one over the other, each of which has a depolarising axis; and as these axes are placed in every direction, the body acquires the property of depolarising in every direction. 4. Those bodies that have an approximation to a neutral axis, as gold-beater's skin. 5. Those that have an approximation to a depolarising axis. 6. Those that allow the light nearly to vanish, but not quite, at every alternate sector, as oil of mace. 7. Those that allow it to vanish entirely at every alternate sector, as calcareous spar, when the light passes through the shorter axis.

At the same meeting part of a paper by Benjamin Travers, Esq. was read, On the Mechanism by which the Eye adjusts itself to different Distances. The paper was drawn up with uncommon clearness, and even elegance. It consisted of three parts: 1. A sketch of the different hypotheses to which this mechanism has been ascribed, with the author's reasons for not admitting them. These are the muscles of the eye, the ciliary processes, the supposed muscularity of the lens itself. 2. An account of the anatomy of the eye, as far as it is requisite for the purposes of the paper. 3. His reasons for believing that external pressure is applied to the lens, and that this pressure is sufficient to alter its shape.

On Thursday 22d Dec. Mr. Travers's paper was continued. He gave his reasons for considering the iris as muscular, and that by means of it the pupil is adjusted to different distances.

The Society adjourned over two Thursdays, on account of the approaching holidays.

LINNÆAN SOCIETY.

On Tuesday, December 6th, a paper by James Johnson, Esq. was read, giving an account of some fossil bones found in the cliff near Lyme, Dorsetshire. This cliff abounds in belemnites, nautili, and the remains of other sea animals. The bones in question have been supposed to belong to the crocodile; but Mr. Johnson gave his reasons for considering that opinion as ill founded. He thinks they constitute the bones of a new and unknown species of amphibious animal. He is of opinion that the animals whose remains are found here lived and died upon the spot.

A communication from Mr. Sowerby was also read, On the Spiral Tubes of some Fossil Shells belonging to the genus Anomia of Linnæus and Terrebratula of Lamark. These shells occur in limestone, and he thinks the spiral tubes will furnish characters sufficient to constitute them a new genus.

M. Vicillot's paper begun at the last meeting was continued.

On Tuesday December the 20th M. Vieillot's paper was continued.

Specimens of flax and hemp were exhibited to the Society, prepared by machinery invented by Mr. Lee, without water steeping or dew retting. The specimens were beautiful. The advantages of the plan are that the produce is greater and better; and the green part of the plant is preserved, which furnishes very good food for cattle, and is an excellent manure. The seed also is preserved.

WERNERIAN SOCIETY.

At the Meeting of this Society, on Saturday the 12th of November, there was read a paper by the Rev. Dr. Fleming of Flisk, On the Fossil Species of Orthocera found in Britain. The Doctor has paid much attention to petrifactions; and he here described no fewer than ten species of orthocera, all of them found in strata belonging to the independent coal formation in the county of Linlithgow. Two of these orthoceratites have already been figured by the late Rev. Mr. Ure, in his History of Rutherglen and Kilbride; the rest were now for the first time described; and the descriptions were illustrated by beautiful and correct drawings. The secretary presented two communications from Mr. Scoresby, junior, of Whitby, of great interest to the meteorologist; being journals kept during voyages to Davis's Straits, in the years 1813 and 1814, in which Mr. Scoresby notes the latitude and longitude; the temperature, generally giving the mean of three daily observations; the direction and strength of the winds; the aqueous and other meteors;

the modifications of the clouds according to Mr. Luke Howard's plan; and the different animals seen, the appearance of the ice, and other incidental circumstances.

At the same meeting there was read a letter from Mr. William Nicol, Lecturer on the Philosophy of Natural History, addressed to Professor Jameson, giving an account of a bed of flexible floetz limestone, observed by him on the coast of Durham county, near Tynemouth castle. Fine specimens were exhibited. This limestone is very distinctly of a lamellar structure. When a thin bar of it is thoroughly wetted, it is very flexible; when evaporation has proceeded a certain length, it becomes rather rigid; but when the water is completely exhaled, it regains its flexibility, nearly to the same extent as when saturated with water.

There was also read a short account of the different strata cut through in sinking the pit to the coal at Brora in Sutherlandshire, communicated by Mr. Dempster of Dunichen.

ARTICLE VII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. SINGER will commence his Lectures on Electricity and Electro-Chemistry, at the Russell Institution, on Monday the 16th of January, at 8 o'clock in the evening. The Lectures will be continued every succeeding Monday at the same hour. A syllabus may be obtained of the secretary, at the institution, Great Coram-street, Russell-square; or of Mr. Singer, N° 3, Princes-street, Cavendish-square.

Dr. Clarke and Mr. Clarke will begin their next Course of Lectures on Midwifery and the diseases of women and children, on Monday, January 23d. The Lectures are read every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals. For particulars apply to Dr. Clarke, New Burlington-street, or to Mr. Clarke, 10, Saville Row, Burlington Gardens.

Medical School of St. Thomas's and Guy's Hospitals.—The Spring Course of Lectures at these adjoining Hospitals will commence the beginning of February, viz.:

At St. Thomas's.—Anatomy and the Operations of Surgery; by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery; by Mr. Astley Cooper.

At Guy's.—Practice of Medicine; by Dr. Babington and Dr. Curry.—Chemistry; by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy; by Mr. Allen.—Theory of Medicine, and Materia Medica; by Dr. Curry and Dr. Cholmeley.—

Midwifery, and Diseases of Women and Children; by Dr. Haighton.—Physiology, or Laws of the Animal Economy; by Dr. Haighton.—Structure and Diseases of the Teeth; by Mr. Fox.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt at the respective Hospitals.

II. *Important Chirurgical Operation.*

A new method of operating for the cure of Popliteal Aneurism has been employed in Dublin with the most complete success, which seems to open new and important views with respect to the treatment of diseased and wounded arteries in general.

The operation was performed by Mr. Crampton, Surgeon General, at the King's Military Infirmary, near Dublin, on the 15th of October. The following particulars respecting the operation are extracted from the Hospital Journal. "The femoral artery was laid bare at the usual place, by an incision three inches in length and *compressed*, (but not tied circularly,) by a narrow tape, so as completely to obstruct the current of the blood *with the least possible disturbance to the artery*, or *injury to its coats.* The ligature was by a peculiar contrivance applied in such a manner as to enable the operator to tighten or to relax it at pleasure, without interfering with the wound. In two hours and a quarter the ligature was gently relaxed, but not completely loosened; no pulsation in the ham. In twenty-four hours the artery was relieved from all compression; but as a measure of precaution, the ligature was left in the wound. In forty-eight hours the ligature was withdrawn, and the wound was united by adhesive plaster."

The patient was examined on the fifth day by several professional gentlemen; his health was not in the least degree deranged. The tumor, which had decreased by one half, was without pulsation, and nearly incompressible. The temperature of each foot was 84°.

On the 14th day the wound was nearly healed, and the man went about the ward on crutches.

On the 18th day the wound was healed, and the tumor which could be seen only in the extended position of the limb, was incompressible, and altogether free from pain.

The advantage of Mr. Crampton's operation consists in its imitating in the most favourable way the process of nature, in the spontaneous cure of aneurism.

1st. By interrupting the course of the blood through the ruptured artery, the fluid contents of the aneurismal sack are allowed to coagulate, and the circulation is thrown upon the collateral branches.

2d. The subsequent obliteration of the artery is effected by a natural process, which protects the patient from the long train of sufferings, and of dangers necessarily attendant upon the permanent contraction of a great artery, and its separation from the ligature by the process of sloughing or ulceration.

The details of this important case will no doubt be speedily communicated to the profession, through some of the usual channels of scientific intelligence.

III. *Air Volcano.*

The Europeans who are not seasoned to the climate are accustomed to leave Carthagena (in South America) during the heats of summer, and to spend that season at Turbaco, an Indian inland village, delightfully situated on the top of a hill. Mr. de Humboldt and his fellow traveller M. Bonpland, spent the month of April, 1801. in this village. During their herbalizations, they were informed by their Indian guides, that there existed a country not far off, in the midst of a forest of palms, to which the name of *volcanitos* (*little volcanoes*) was given; that its surface had been formerly covered with fire; that a priest had extinguished the combustion with holy water; and that now it emitted nothing but water. On going to the spot, our travellers found in the middle of a vast plain, eighteen or twenty small cones, about 24 feet in height. These cones are formed of a blackish grey clay, and have an opening at their summits filled with water. On approaching these small craters, a hollow but very distinct sound is heard at intervals, 15 or 18 seconds previous to the disengagement of a great quantity of air. The force with which this air rises above the surface of the water, may lead us to suppose that it undergoes a great pressure in the bowels of the earth. Five explosions generally take place in two minutes, and this phenomenon is often attended with a muddy ejection. According to the Indians, the forms of the cones undergo no visible change in a great number of years; but the ascending force of the gas, and the frequency of the explosions, appear to vary according to the seasons. Mr. de Humboldt examined this gas, and found that it was azote mixed with less than one thousandth of its bulk of oxygen gas. Here is a phenomenon well worth the attention both of geologists and chemists. Whence comes this azotic gas? How is it produced? Does there exist in this place an immense magazine of ammonium, which by means of water is converted into azote? We are far indeed from being able to philosophize in the present state of our knowledge, respecting the changes that take place under the surface of the earth.

IV. *Mathematical Problem.*

(To Dr. Thomson.)

SIR, *Plymouth, Nov.* 21, 1814.

I beg, through the medium of your Journal, to present to the attention of the mathematical part of your readers, the following

Theorem, originally proposed by the celebrated John Bernouilli, and which I have exracted from Bossut's Calcul. Integral, tom. i. p. 484.

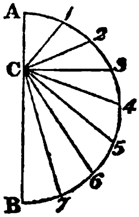
The learned author, from whose invaluable work I have taken it, has not given the investigation; and I therefore solicit the favour of your inserting it, in hopes that some of your correspondents may be able to furnish a solution to it.

I am, Sir, your humble Servant,

GEORGE HARVEY.

Theorem.

"Let A 4 B be the semi-circumference of a circle, of which A B, the diameter, is equal to the two semi-axes, A, C, B C, of a given elipse. Divide the semi-circumference, A 4 B, into a number, 2, 4, 8, 16, &c., of equal parts; draw straight lines from the point C to all the points of division: take the arithmetical mean of all the straight lines drawn to the points of division denoted by odd numbers, and also the arithmetical mean of the sum of all the lines drawn to the points of the even numbers, and the half of A B; these two arithmetic means shall be the radii of two circles, the circumference of one of which shall be less than that of the elipse, and the circumference of the other greater."



Required the demonstration?

V. *Burning Gas in the Appenines.*

Sir Humphry Davy lately discovered, in the Appenines, a jet of gas burning with great brilliancy, and forming a column of flame six feet high. The gas was pure carbureted hydrogen. This is a fact of considerable interest in a geological point of view. The Appenines consist chiefly of limestone, and belong, unless I am misinformed, to the floetz formation. It would be of importance to know, whether any coal exists in the neighbourhood of this flaming jet of gas, or whether it proceeds from a great depth under the surface. This gas is formed in great abundance at the bottom of dirty stagnant water during the summer time. But that fact throws no apparent light on the formation of the gas in the mountains of the Appenines.

VI. *Weather in Iceland during the Winter* 1813—14.

Some of my meteorological friends are anxious to know through the medium of the *Annals of Philosophy*, whether there was any thing peculiarly severe in the last winter in Iceland. If any of my readers happen to be acquainted with the matter of fact, it would be considered as an obligation if they would communicate what they know.

VII. *Chromate of Iron.*

Chromate of iron occurs in the Bare Hills near Baltimore, in North America, in great abundance. These hills appear to be serpentine, so that the situation of this mineral is the same in America as in France. The chromate of iron occurs in these hills in three states: 1. In compact masses of considerable size, mixed with talc, steatite, &c. In this state, which is I presume the same nearly with the French chromate, it has some resemblance to brown blende. 2. In grains from the size of a mustard seed to that of a pepper corn, imbedded in steatite. 3. Crystallized in regular octahedrons. The colour of these crystals is black, inclining to steel grey. Surface smooth. Lustre vitreous. Fracture conchoidal. Opake, except some specimens which transmit a blood red light. Infusible before the blow-pipe, without addition. Magnetic in a considerable degree. Mr. Hayden, from whom this account is taken, believes that many specimens of supposed octahedral iron ore are really chromate of iron.*

VIII. *Magnetic Pyrites.*

Stromeyer has lately subjected this mineral to a new analysis, and as he was at uncommon pains to attain the mineral in a state of purity, it is probable that the result which he obtained is a close approximation to the truth. Mr. Hatchett published the first analysis of this mineral, and made us acquainted with its true nature. It was analysed by Proust, and experiments on the artificial combinations of iron and sulphur were made by Vauquelin. The following is the result which they obtained:—

	Iron.		
Hatchett	100	+	57·48 sulphur.
Proust	100	+	60
Vauquelin	100	+	28·22
Stromeyer	100	+	67·08

If my number for the weight of an atom of iron be correct, and if magnetic pyrites be composed of one atom iron and two atoms sulphur, its constituents should be

Iron.
100 + 55·99 sulphur.

This circumstance throws some doubt upon the accuracy of Stromeyer's analysis. He informs us, that magnetic pyrites usually contains a mixture of metallic iron and of cubic pyrites. If this be the case, the chance of error in such an analysis was considerably increased.

IX. *Whitestone.*

Among some minerals which I lately received from Freyberg, there is a specimen of the true whitestone of Werner. This rock

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* Bruce's American Journal, No. iv. p. 243.

I find is an aggregate of three different minerals; namely, *felspar, quartz,* and garnet. The felspar and quartz are both white, and constitute the greatest part of the rock. The garnet is scattered through the rock in small red grains and crystals. From this specimen it appears that our notions respecting this rock in this country have been hitherto incorrect. I have never seen any rock resembling it in Great Britain, except in cabinets; and in no one case was I able to learn whence the specimen had come. We see too that Klaphroth's analysis, given in a preceding number of the *Annals,* is of no real utility, as the constituents so found must depend upon the accidental proportions of the three minerals constituting whitestone.

X. *Height of the Peak of Teneriffe.*

Of all the measurements of this mountain, there seem to be only three upon which we can rely. Borda measured it trigonometrically in 1776; Lemanon barometrically in 1785; and Cordier barometrically in 1803. Their results are as follows:—

Borda	1905 French toises.
Lamanon	1902
Cordier	1920
Mean	1909

This gives us the height of the peak above the level of the sea, 12,207 English feet.

XI. *Dip of the Magnetic Needle, and Intensity of the Magnetic Forces in the North Atlantic in 1799.*

N. Latitude.	W. Longitude from Greenwich.	Magnetical Dip.	Number of oscillations in ten min.	Remarks.
38° 52′	18° 42′	68·18°	242	Good observation.
37 26	18 52	67·81	242	Almost perfect calm.
34 30	19 15	65·70	234	Perfect calm.
31 46	19 24	64·71	237	Doubtful, especially the intensity.
28 28	20 53	62·41	238	Good.
24 53	23 18	60·84	239	Very good.
21 29	28 2	58·18	237	Good.
19 54	31 5	57·27	236	Good.
14 15	50 23	50·67	239	Good.
13 2	55 35	45·60	234	Dip good, intensity doubtful.
11 1	57 11	42·34	237	Good.
10 46	63 14	42·25	229	Good.

Humboldt's Personal Narrative, ii. 115.

XII. *Postman of the Province of Jaen de Bracamoros.*

In order to render the communication between the coasts of the Pacific Ocean, and the province of Jaen de Bracamoros, situated on the east of the Andes, more easy, the postman of Peru descends swimming for two days, first the river Guancabamba, or Chamaya, and afterwards that of Amazons, from Pomahuaca and Ingatambo to Tomependa. He wraps the few letters of which he is the bearer every month, sometimes in a handkerchief, sometimes in a kind of drawers called guayuco, which he winds as a turban round his head. This turban contains also the great knife with which every Indian is armed, rather to cut his way through the forests, than as a weapon of defence.

The Chamaya river is not navigable, on account of a great number of small cascades. Its fall, according to Humboldt, from the ford of Pucara to its mouth in the river of the Amazons, below the village of Choras, a distance of only 18 leagues, is 1778 English feet. The postman in order to fatigue himself less in descending the river, supports himself on a log of bombax or ochroma, trees of very light wood. When a ledge of rocks intersects the bed of the river, he lands above the cascade, crosses the forest, and re-enters the water when he sees no further danger. He has no need of taking provisions with him, for he finds a welcome in a great number of huts, surrounded with plantain trees, and situated along the banks of the river, between las Huertas de Pucara, Cavico, Sonanga, and Tomependa. These rivers are not infested with crocodiles; the savage hordes therefore almost all travel like the Peruvian postman.—*Humboldt's Researches*, p. 254.

XIII. *Floetz Trap Rocks.*

It appears from the observations of Professor Jameson that there exist three floetz trap formations: 1. That in old red sandstone. 2. That in the independent coal. 3. The newest floetz trap, which is considered as newer than the other two. It constitutes the floetz trap formation described by Werner. The floetz trap mountains of Scotland belong chiefly to the two first formations.

XIV. *Granite in Scotland.*

Two granite mountains have been lately discovered in Roxburghshire, immediately on the borders of England. They appear to be situated in the midst of the transition rocks of that country.

XV. *Hoffman.*

Hoffman, well known as the author of the best German work on mineralogy, is just dead. Unfortunately he has left his work unfinished.

XVI. *Steffens.*

Dr. Steffens, one of the most eloquent teachers of natural history on the Continent, is a professor at Breslaw, the capital of Silesia. When Silesia was invaded by the French armies, he called together his class, pointed out to them in glowing colours the disgrace of quietly witnessing the pollution of their country by an enemy, and the honour to be obtained by joining in the glorious

REMARKS.

Eleventh Month.—4. Misty morning: cloudy: shower at night. 5. Overcast, a.m.: wet afternoon and evening. 6. The drops of dew were frozen, but so clear as not to appear like hoar frost: a fine day, with large *Cirri*, and afterwards *Cirrostratus* and *Cumulus*: a streaked orange sky at sun-set. 7. Hoar frost, followed by cloudy sky: some rain after dark. 8. Clear morning: hoar frost: about four p. m. the sky became very dark, and there was a storm of rain, attended by a single loud explosion of electricity. It is remarkable that on the same day of the month last year, at the same hour, and in the same direction, (south-west,) we had a similar storm. 9. *Cirrus*, with the compound modifications: several shooting stars this evening: windy afterwards. 10. A steady breeze from the N.W.: *Cirrostratus* appeared over the Thames at sun-rise: ice a quarter of an inch thick, and permanent: a brilliant twilight; first lemon colour, surmounted by purple; then, by the blending of the two, a rich deep orange. 11. Hoar frost. I observe this often continues an hour longer on some tufts of *saxifraga cæspitosa* than any where else in my garden: the plant grows on close spongy masses of fibre, which are bad conductors of heat. 12. It was stormy last night; and is said to have thundered and lightened about three a. m. Wet this morning; but the day fine: twilight orange, delicately varied with dusky horizontal striæ. 13. Cloudy and raining at eight a. m., but fine afterwards, with a smart breeze. 14. Cloudy. 15. *Cirrostratus*, a. m.: then *Cirrus nimbiformis*, depending and curling beneath: a turbid sky ensued, and wind in the night. 16. A wet stormy morning. 17. Much wind by night. 18. Small rain, a. m.: wet and stormy at night. 20. Hoar frost: a little rain at intervals. 21. Overcast. 22, 23. Hoar frost: misty. 24. The same: near sun-set a hollow wind from S. W., and rain in the night. 25. Wet day. 26. Some rain, a. m. 27. Clear morning, with evaporation: but before noon cloudiness was coming on from the W., and it rained steadily from sun-set till late at night. 28. Wet again in the night. 30. Large elevated *Cirri*, with *Cirrostratus*.

Twelfth Month.—1. Brisk wind at N.: in the night a slight sprinkling of snow. 3. Wet morning.

RESULTS.

Prevailing Winds, Westerly.

Barometer: Greatest height....................30·28 inches;
Least.............................29·12 inches;
Mean of the period ....29·635 inches.

Thermometer: Greatest height ....54°
Least.......................................19°
Mean of the period ....39·05°

Rain, 3·03 inches. Evaporation, 0·51 inch.

⁂ In the period last reported, the amounts of evaporation were put, by an oversight, in the column headed "Rain," and *vice versa;* which the reader is requested to correct.

Tottenham, *Twelfth Month*, 1814. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1815.

Article I.

An Essay on the Shapes, Dimensions, and Positions of the Spaces, in the Earth which are called Rents, and the Arrangement of the Matter in them. By Mr. John B. Longmire.

(To Dr. Thomson.)

SIR,

I COMMENCED in the year 1812 to write an essay on rents, which I advertised in the beginning of the year 1813 as a work "the object of which was to prove, that metallic veins, dykes, slips, and all other rents, in the internal part of the earth, were formed when it was passing from a fluid to a solid state, and are owing to the unequal contraction of its matter; and that the phenomena of stratification, and formations, in some points of view, as well as the features of the earth at its surface, are effects of the same cause." This work I intended to publish in the beginning of the present year; but since advertising, I have extended it much beyond what I originally intended; and as I purpose to make considerable additions to it, perhaps a few years may elapse before I publish it; but as some of my friends and correspondents have requested its early publication, I have determined, with your permission, to give an abstract of it in the *Annals of Philosophy.* In doing so, I will commence with that part which relates to rents.

I am, Sir, with great respect,

Your very humble servant,

JOHN B. LONGMIRE.

Troutbeck, near Kendal,
Oct. 30, 1814.

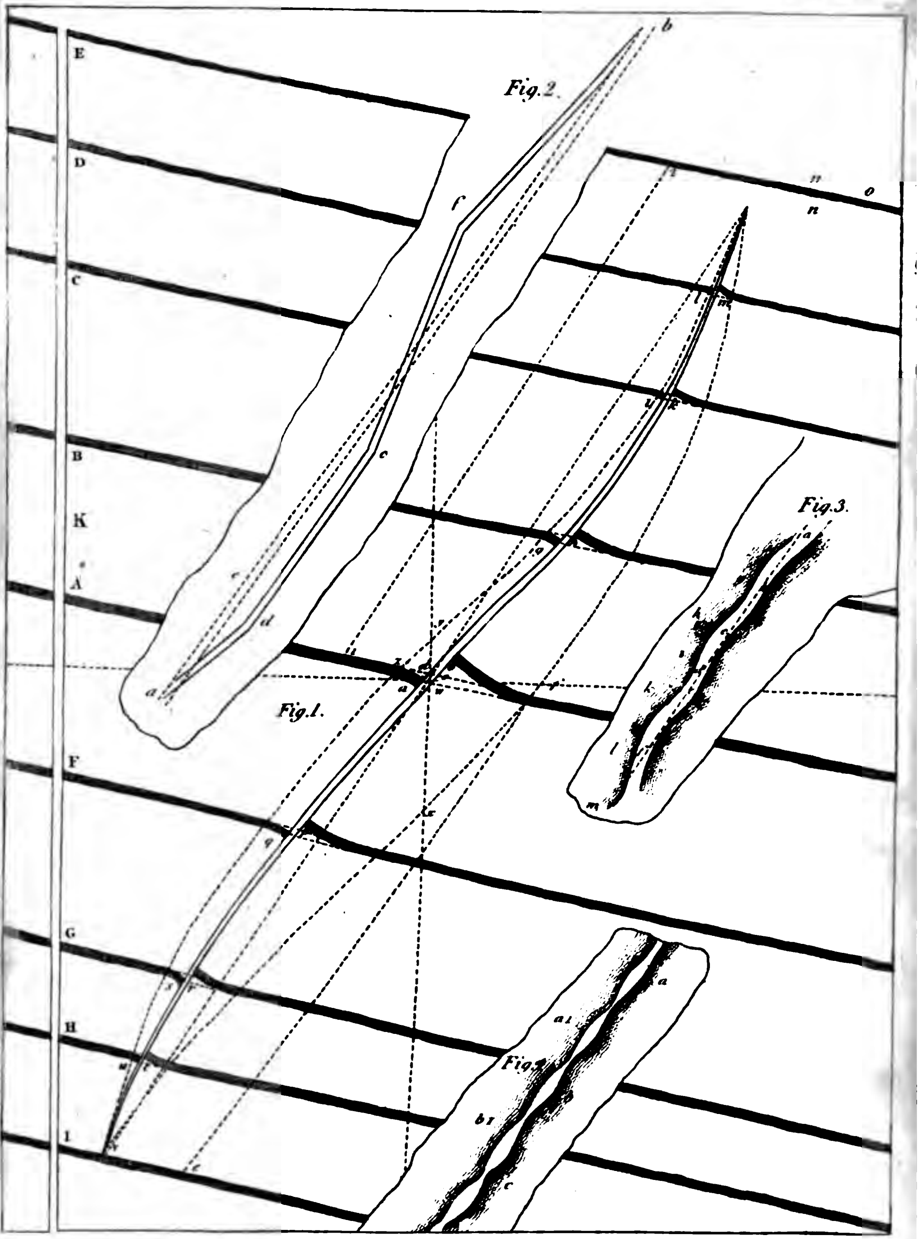
Of Rents in General.

There are numerous spaces existing throughout the earth, many of which extend further from its surface towards its centre than man has hitherto penetrated. These spaces contain irregularly-shaped *tabular masses* of earthy and metallic matter, which have a position very different from that of the matter filling up the other parts of the earth. A close examination of all the phenomena presented by these spaces convinces the observer that they have been formed when the matter of the earth changed its state from fluidity to solidity, and are owing to its contraction unequally. The most of these spaces, or those which were formed the earliest, were filled by some of the matter which is contiguous to their sides being forced into them, from portions of it still nearly in a fluid state, by the weight of the matter incumbent on these portions; the rest of them, or those which were formed when every portion of the matter contiguous to their sides was so far advanced to its present state of solidity as to resist the incumbent weight, were filled with matter that entered them at the earth's surface, in either a fluid state, as the matter of green-stone, basalt, &c.; or a solid state, as gravel, sand, and clay. These spaces, therefore, are rents; and may be called, *according to their shapes*, the bended-tabular, the straight-tabular, the ovalar, and cylindrical, rents.

Of the Bended-Tabular Rent.

In treating of this rent I will, in the first part, describe its common names, its shape, dimensions, and position; the difference in the appearance and position of the strata, between where they are contiguous to it, and at given distances from it; and, lastly, the forming cause of the rent. In the second part, I will describe the arrangement of the matter in rents of this shape; dividing them into such as contain matter which proceeded from that on their sides, and such as have been filled from above.

1. *The Names common to this Rent.*—Bended-tabular rents which contain only earthy tabular masses, and metallic and earthy crystals, are known by the appellations of slips, dykes, shifts, lodes, troubles, and faults. They are denominated slips by some geological writers, because the strata on one of their sides have slipped from those on the other, and fallen below them. They are known at Whitehaven by the names of dykes, because they divide the seams, or bands of coal, as they are there called, into *fields.* They also call them *up-throw* and *down-throw* dykes; up-throw dykes, when the strata are higher on the side opposite to that on which the observer is standing; and down-throw dykes, when they are lower on the former than on the latter side. They are called shifts in some parts of England and Scotland, as they are considered by the majority of miners in these parts to have shifted the strata on their sides. In Cornwall they are denominated cross lodes; and in some



Mr LONGMIRE on RENTS (1)

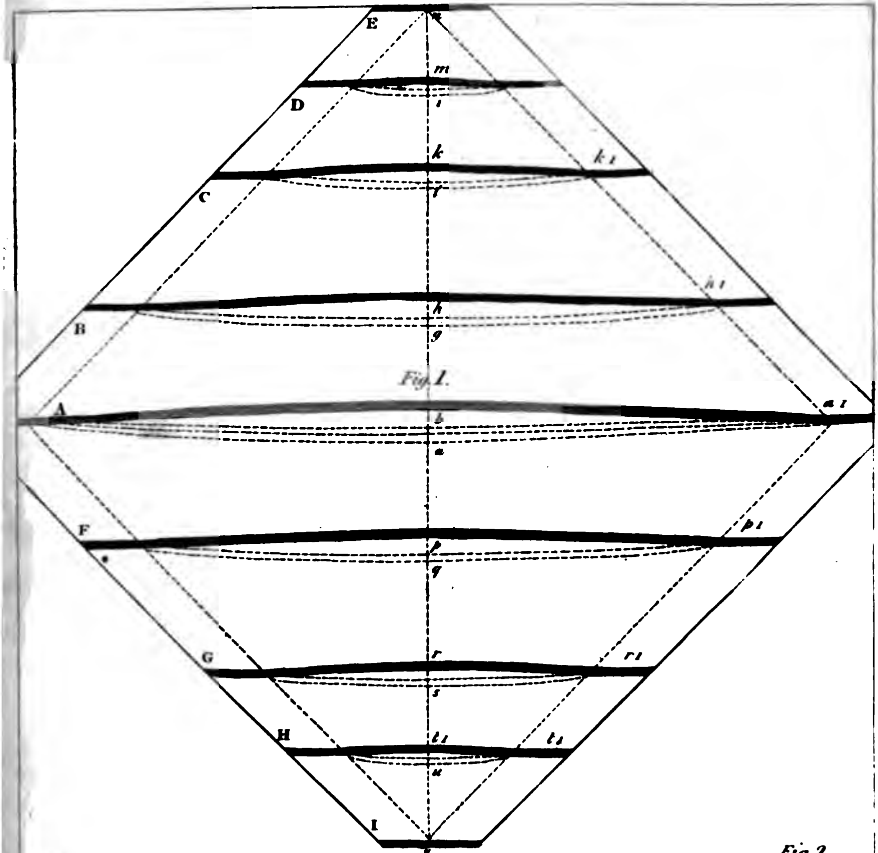


Fig. 1.

Fig. 2.

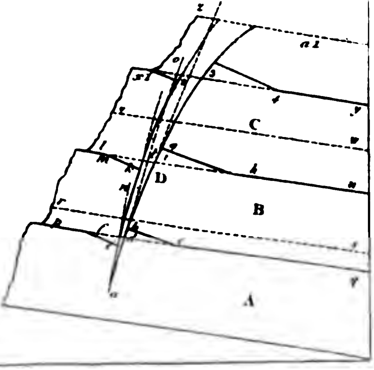
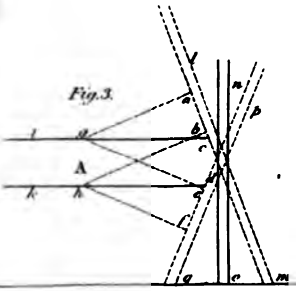


Fig. 3.



Mr. LONGMIRE on RENTS (2)

parts of England and Scotland they are called troubles and faults. Bended-tabular rents, containing earthy and metallic tabular masses and crystals, are generally called metallic veins; and in Cornwall, metallic lodes.

2. *The Shape of this Rent described.*—The shape of this rent is as follows:—Let fig. 1, Plate XXIX., represent a perpendicular view of its angular direction in a situation which is in the middle of its horizontal direction; let the empty spaces between the strata E, D, C, B, &c. be supposed to be filled with strata also, but which are not represented, to avoid confusion in the figure; and let the strata be continued uninterruptedly from the rent, *n w v*, to the pit, K. Then if a miner be excavating the stratum E, he may proceed a considerable distance in it, say beyond the point *o*, without meeting with a rent; but in excavating the stratum D, he may meet with one at *l*; and if so, he will meet it in the strata C, B, A, F, G, H, at *i*, *g*, *a*, *q*, *s*, *u*, and in the stratum I, at *v*; there it terminates, probably near the upper surface of this stratum. Let a straight line, *n f*, be drawn from the highest to the lowest extremity of the rent. This line passes through the rent at its centre, *w*, only, for the rent's upper half is a slightly curved line, *n m h l w*, which lies before the straight line; and the lower half is a similarly curved line, *w p r t v*, that lies above it. Again, let fig. 1, Plate XXX., represent this rent as a person sees it in approaching its upper side; and let the parts of fig. 1, Plate XXIX., be referred to by the same letters: then the parts of the strata shaded dark in the latter figure are on the under side, *n*, *k*, *p*, *v*, of the rent; and the parts represented by the pricked lines only, are on its upper side, *n*, *g*, *q*, *v*. The horizontal direction of the rent is greatest opposite its centre, or opposite the stratum A; there let it terminate at A and *a* 1: then if this distance be bisected, and a line, *n v*, drawn at right angles to the points A, *a* 1, it will bisect the angular direction of the rent. The horizontal direction of the rent decreases in length upwards and downwards from this stratum, with such a ratio, that if four straight lines be drawn, two upwards till they meet in the line, *n v*, at *w*, each meeting it at an angle of 45°, and two downwards, till they meet the same line at *v* under angles of 45° also, these lines will enclose a square figure; whose position is such, that two of its opposite angles, A and *a* 1, are situated both on the same horizontal line, and the other two, *n* and *v*, both on the same perpendicular line. There is a general increase in the width of this rent in every direction, from the lines that join together its sides to its centre. This, therefore, is a square bounded bended-tabular rent; whose width is greatest at its centre, and decreases to the lines that join together its sides.

Such is the general shape of every bended-tabular rent; but particular parts of the angular directions of these rents deviate more or less from the curved line just described, and their horizontal directions deviate also from straight lines, while the widths of these and other parts vary much from that general increase which begins at their lines of termination and ends at their centres.

Both directions of this rent are crookedly and curvilinearly bent. I will describe two of the principal bends. Let the dotted lines, *a*, *b*, fig. 2, Plate XXIX., represent the angular direction of a part of this rent, which being a small part of the whole, reduced to so small a magnitude, when compared with its real size, the curve peculiar to this direction cannot be observed. Suppose the rent's angular direction in the first part make an angle, *c d a*, of about ten degrees with its general direction, *a b*; it may then be parallel to this direction a greater distance, *d c*; then incline to, and cross it, making with it as small an angle as before, during a greater or less distance, *c f*; it then inclines to and re-crosses it, making only a small angle with it, for a considerable distance, *f b*. These bends are common to the horizontal, as well as the angular, direction of this rent, and they may be thus accounted for: The parts which differ in appearance, consistence, &c. from the characteristic parts of the strata, originally laid in these crooked directions; and as these parts caused the rents in a manner to be afterwards described, the directions of these rents must necessarily follow those of such parts. Both directions of these rents are curvilinearly bended, generally as fig. 3, Plate XXIX., represents.

The variations in the width are mostly similar to those described as follows: The width of this rent will often decrease opposite a few strata till the sides are nearly in contact; it will then increase opposite a few more strata, till it be as great or greater than before; it will then decrease and increase alternately, with different ratios, during which its sides will occasionally meet, and remain in contact for variable distances. The widths of both directions of a rent are subject to these variations; and either of them will be well represented by fig. 4, Plate XXIX., which in this instance is a part of the angular direction.

3. *The Dimensions of Bended-Tabular Rents.*—Bended-tabular rents exist of all dimensions, from a few feet to several hundred yards in both directions; but their widths are generally from two to four feet, and seldom exceed four yards, although there are a few instances in Cornwall of their swelling out to ten yards. The following may be considered large rents: Two or three rents have been traced in, and beyond, the Whitehaven coal-mines about 3000 yards in their horizontal directions; and one called Saltom North Dyke is known to be more than 550 yards in its angular direction; but how much more cannot be now ascertained: and Professor Jameson mentions two near Freyberg, in Germany; one of which is more than four miles, and the other more than ten miles, in this direction. Now the extent of the largest mine, whether one of those which are used for the abstraction of ores out of rents, or useful minerals from the strata themselves, is far from being equal to the larger of these rents; of course, the observations of miners

cannot have been so numerous, nor so perfect in their nature, as to describe with sufficient accuracy the general laws, much less the peculiarities of the laws, respecting the figures of the larger rents, and other necessary particulars; but many mines, probably, are sufficiently large to enable skilful miners to determine the particulars necessary to describe all phenomena peculiar to the smaller rents, and to show that the larger correspond with the smaller in every particular known of the latter rents. This is particularly the case with the Earl of Lonsdale's coal-mines at Whitehaven,* in which I made the many new observations respecting the shapes and positions of this rent, and the peculiar arrangement of the strata contiguous to them, which will appear in these essays.

4. *The Positions of Bended-Tabular Rents.*—The most of these rents have angular positions; but some have a position which is angular in one part and perpendicular in another; others, one that is angular in one part and horizontal in another; and others, one which is angular in one part, perpendicular in another, and horizontal in a third. The positions of these rents depend on those of the strata which form their sides; if, as Williams says, "the strata lie in a horizontal position, the fissure of the slip," or the position of a rent, "will lie nearly perpendicular; but if the strata hang with any considerable degree of declivity, the fissure of the slip will be found in a slanting direction, as all these sections cut transversely, or right across the strata, or nearly so." This law arises from that which follows: The surfaces of separation of the strata contiguous to the sides of this rent are at right angles, or nearly so, to the upper and under surfaces of these strata. Therefore when the stratum A, fig. 3, Plate XXX., is horizontal, as when it is represented by the letters *i g c d h k*, its small surface, or surface of separation, of which *c d* is the perpendicular direction, and which is at right angles to the stratum's upper and under surfaces, will give a perpendicular position to the rent, *n o*, of which it forms a part of one side. On the same principle, when the stratum A, is bent upwards till it acquire the position represented by the letters *i*, *g*, *a*, *b*, *h*, *k*, it will give an angular position to that part of a rent, (the part *l m*,) where it forms a part of one of its sides. If this stratum be bent downwards till it be represented by the letters *i*, *g*, *e*, *f*, *h*, *k*, the side of the rent, *p q*, of which it forms a part, will be angular also; but in this instance the angular direction

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* In the Whitehaven coal-mines three strata of coal are worked: the first, the thickness of which varies from four to six feet, is called the Bannock Band, and appears at the earth's surface at the highest part of the mine, but is more than 90 fathoms below it at the lowest part: the second stratum, whose thickness varies from six to twelve feet, is called the Main Band, and is situated at a distance below the highest band, that, if taken in different places, will vary from 11 to more than 20 fathoms: the third, the Six Quarters Band, is from four to six feet in thickness; and is situated in some places 30, in others 40, fathoms, below the Main Band; and in others any distances between these extremes. The surface area, under which the first two named bands are excavated, is not less than from ten to twelve square miles. This is an area of mine, not to be equalled, I believe, by that of any other proprietor.

points to the reverse side of a perpendicular line, *n o*, to that to which the angular direction in the second instance points; and if the stratum be bent till it be perpendicular, the side of the rent where its surface of separation is situated will be horizontal.

5. *The Difference in the Position and Appearance of the Strata between where they are contiguous to, and at given Distances from, these Rents.*—The difference in the position of the strata is such, that on the under side of all rents of this shape they are bent upwards toward these rents; and on their upper sides, downwards toward them. This is the reason why miners never meet with two parts of one stratum *opposite each other* on reverse sides of a rent of this shape, but one part always a variable distance above the other. The part of a stratum is very little bent downwards, to what it is bent upwards, towards this rent. Both parts of a stratum are bent: the greatest distance opposite the middle part of a rent; and the least, near its highest and lowest extremities; and they are also the least bent near the horizontal extremities, and the most at the centre of this rent. These positions will be better understood by having reference to a diagram. The part, *b f*, of the stratum A, fig. 1, Plate XXIX. which is opposite the centre of the rent, *n w v*, is bent upwards; the parts *h*, *k*, *m*, of the strata B, C, D, are also bent upwards; but the further they are individually situated from the middle stratum the less they are bent. The same rules hold good with the parts of the strata *p*, *r*, *t*. Again, the part *z a* of the stratum, A, on the upper side of the rent, is bent downwards a very little, but more than any of the strata, either above or below it, as the ratio of the strata's bending decreases upwards, and downwards from the stratum A, to the strata E and I, which are not bent near this rent. Again, the surfaces of separation of the stratum A, are the greatest distance asunder at *a b*, fig. 1, Plate XXX., or opposite the centre of the rent; and the least, near its horizontal extremities, A, *a* 1. The same rule applies to all the strata opposite this rent, either above or below the stratum A.

The appearance and hardness of that part of every stratum which is close to this rent are different to the part of the same stratum that is situated more remotely from it. These differences are owing to corresponding differences in their proportions of elementary matter; for on examining the strata we find one part of them has sometimes one class* of elementary matter, and often one denomination, or more than one, of the class which the other parts want; and that the proportions of the different denominations of the classes peculiar to both parts are different in one to what it is in another part of a stratum. This arrangement of the elementary matter of a stratum is found in different parts of all strata, but is most observable near rents. These differences have been the most accurately examined in common coal. Sometimes we find that part of a stratum of coal which is contiguous to this rent contains only carbon and small portions of iron, and earthy matter; while the parts more remote contain carbon, *bitumen*, and the other ingredients peculiar to common coal. Again: near this rent the coal often contains ten per cent. of earthy matter; but this is not the case with it further from the rent: and coal sometimes has its portion of iron increased to five or six per cent., but wants its bitumen; then its appearance and consistence approaches very near to those of graphite, or black lead.* The differences in the appearance of one part of every stratum, when compared with that of another, is very various; and the nature of these differences in different strata is as various; but in this particular they all agree with one another, that the parts near the rents which differ from the *characteristic* parts of the strata have *the least degree of hardness, and would be the least able to resist any force that may have acted against the attraction of cohesion.* All the strata are not wholly different near this rent to what they are at given distances from it. We pass through a rent in many places near its centre, sides, or higher or lower extremities, without perceiving an alteration in the strata on either side of it; but after much examination of them, I have satisfied myself that there is the following general law respecting them: the parts which are different to the rest on the under side of the rent are generally in the greatest abundance opposite its lower half, but these parts are opposite the higher half on the upper side of this rent. In fig. 1, Plate XXIX., the lines *b f*, *f e*, and *e v*, will represent the extent of the differing parts opposite the lower half, and the lines *a* 1, 12, and 2 *n*, the extent of these parts opposite the upper half of this rent.

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* The classes of the earth's elementary matter are generally divided into four—earths, metals, inflammables, and salts. The different denominations of these classes may be silex, alumina, &c.; iron, copper, lead, &c.; or bitumen, carbon, &c.

6. *The forming Cause of this Rent deduced from the foregoing Facts.*—When we consider that the positions of the strata above and below the small bended-tabular rents, have not been altered by the force which altered the positions of those on their sides, and that it has altered the positions of these for small distances only, we must conclude, that had it acted either from above or below the rents, or from either of their sides, it could not have formed them, by producing such alterations in the positions of the strata adjoining them. We must, therefore, define the forming cause of this rent, one that has acted only in the altered parts of the strata. This cause *could* be no other than the matter's contraction, unequally, in changing its state from fluidity to solidity.

During the change of the matter from fluidity to solidity, there would be a period of time when its consolidating process was so far advanced towards completion that if any separations took place in it the parts into which it was separated could continue so. At that time the attraction of cohesion in the middle parts of strata could not be so powerful as to draw the matter towards their centres as they diminished in bulk; they would, therefore, be gradually separated into masses of such dimensions that the attraction of cohesion could draw the matter of each towards its centre from every side as it continued to contract.

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* Many such differences in other strata have been described by mineralogists. The parts which differ very much from the rest they have supposed to be parts changed by some chemical agent; but I consider them as *original* differences, and the cause of all rents; because the strata, in the parts very different from the rest, would diminish in bulk, *less or more* in the former parts than in the latter, as is afterwards described.

The *diminution of bulk*, or the *contraction* of the strata, is very evident from the rents or spaces between the numerous masses into which they are divided. The contraction of strata horizontally can be exactly ascertained. Thus if the length of the curved line, *f b*, be taken and laid in the direction *f z*, it will reach to *c*, and the curve line, *z a*, similarly disposed will reach to *d*, so that there will be a distance, *d c*, between them, which is the *horizontal contraction* of the part, *z f*, of the stratum A. A distance between similar parts of all other strata on the sides of this rent is also observed; the length of which is more or less, according as its situation is more or less distant from the centre of the rent. And although we have no *direct* proof that the strata have contracted perpendicularly; because in this direction its weight has no tendency to counteract the force of cohesion, therefore no separation would take place; yet as the strata have contracted horizontally, we must infer their contraction perpendicularly, as it is impossible to conceive that they could contract in the former, and not in the latter direction. But, as will be afterwards shown, because the contraction horizontally is greater in one part than in another, it is equally probable that the contraction perpendicularly would be as unequal. This is precisely the case: for a stratum lies in different levels on opposite sides of all rents of this shape. Hence the strata on one have contracted a greater distance perpendicularly than on the other of their sides.

The formation of the rents of this shape by the contraction, unequally, of the matter contiguous to their sides is a consequence resulting from the arrangement of the strata near them; but the differences in the classes, and the proportions of each class of elementary matter of the parts of the strata near the sides of rents, when compared with those of these at given distances from them, gave rise to the unequal contraction of the strata; for the parts of strata which differ from others in the proportions of their usual classes of elementary matter must have contracted more or less than them; and those also which have a different class of matter, and different proportions of the classes common to both, must have contracted more or less also. Thus if the part of a stratum of coal, with its usual proportions of elementary matter, was joined to a part wanting a considerable quantity of bitumen, but had an extra portion of earthy matter to make up this deficiency; these two parts would not contract equally in passing from a fluid to a solid state, and of course they would separate where they joined each other.

Many instances might be brought forward to show the cause of the unequal contraction of every stratum; but the one adduced is sufficient to establish the principle. Having made these premises on the nature of the unequal distribution of the elementary matter of strata, I will now show the mechanical part of the process by which it produced the peculiar arrangement of the strata near the bended-tabular rent; and, in consequence of it, the formation of this rent.

Suppose the state of solidity of the matter such that the formation of rents had commenced, and that the rent, D, fig. 2, Plate XXX., was produced in a place where the matter's attraction of cohesion was least able to resist the separating force. Let the dotted lines, *r s*, *v w*, *z a* 1, be the upper sides of the strata A B C, when the formation of the rent, D, commenced; and the lines *p f e b c q*, *t m k g h u*, and *x* 1,2 3 4 *y*, those of these of the strata when they had acquired their present state of solidity. The formation of the rent first commenced in the stratum A; and after the surfaces of separation were formed, the part *b c* contracted perpendicularly less, and the part *f e* a very little more, than did the stratum A, in general. In consequence, then, of the contraction, horizontally and perpendicularly, of this stratum, the rent *b a e* was produced; whose under side, *b d*, is at right angles to the part *b c*, and whose upper side, *a e*, is at right angles to the part *f e*; and as the end, *e*, of the part *f e* is not so far below the line *f c* as the end, *b*, of the part *b c* is above it, the angular direction of the sides, *b a* and *e d*, of the rent, D, meet at the point, *d*, from a width, *e d*, at their upper extremities that is nearly equal to the horizontal contraction of the part *f c* of the stratum A. The arrangement of elementary matter in the stratum B being similar to its arrangement in the stratum A, a rent would take place in the former immediately above that in the latter stratum. Like the stratum A, the stratum B would contract less in one part, *g h*, than in another, *m k*; but although its thickness be the same as that of the stratum B, the distance *g i* is twice the distance *b d*; because, besides that it has contracted as much less in that part than in other parts, like the stratum A, the progress towards the present state of solidity of the stratum A was greater in the same period of time than that of the stratum B; consequently the part *c b g h* of this stratum was raised as much above the line *d c* as the line *b c* is above the line *f c*, or as much as it contracted less in that part than in general. In such a manner were the parts *t*, *r*, *p*, *b*, of the strata H, G, F, A, fig. 1, Plate XXIX., raised above the other parts of the same strata. Again, the distance which the point *k* is below the line *m h u*, fig. 2, Plate XXX., is twice that which the point *e* is below the line *f c*; because the part *m k e f* of the stratum B has contracted as much as the same part of the stratum A, and is as much let down below the other parts of the stratum B, by the unequal contraction of the stratum A, as the point *e* is below the line *f c*. In this manner were the parts *u*, *s*, *q*, *a*, of the strata H, G, F, A, fig. 1, Plate XXIX., allowed to descend below the other parts of the same strata. But as the ratio of contraction of the parts *h*, *k*, *m*, *n*, of the strata B, C, D, E, is as much less than the general ratio of all the strata on the under side of this rent, as that of this of the strata below the stratum A is greater than it; so the elevation of the first stratum above the stratum A would not be so much as the elevation of this stratum by the difference of the ratio; nor would the third stratum above the stratum A be so much elevated as the second; nor the fourth, as the third; so that the elevations of the strata above the stratum A, as their distances from it increased, would be less; till at last no alteration in their positions could be observed. Again, as the ratio of contraction of the parts *g*, *i*, *l*, is as much greater than the general ratio as that of this of the strata below the stratum A is less than it; so the first stratum above this stratum would not be suffered to descend so much as it by the difference in the ratio; nor would the third stratum above the stratum A descend so much as the second; nor the fourth, so much as the third; so that at the distance above the stratum A which is equal to that between this stratum and the lowest extremity of the rent, no alteration in the general position of the strata would take place.

Upon this alteration in the position of the strata depend the *shape* and *position* of these rents. Let the line *b o*, fig. 2, Plate XXX., be a continuation of the angular direction of the separated surface of the stratum A on the under side of the rent D; then as the line *g h* makes an angle, *g i*, with the line *k m* that is greater than the angle *b c d*, which the line *c b* makes with the line *c f*, the last of which is parallel to the line *m h*: so an angle, *o g*, is produced, which is the deviation of the stratum B's surface of separations' angular direction from that of the separated surface of the stratum A; and as the angle *l m k* is greater than the angle *f e d*, so the angular direction, *e k*, of the stratum B on the upper side of the rent makes an angle, *k e n*, with that of the stratum A, which, being less than the angle *o b g*, the rent is wider at *l i* than at *d e*. Again, the angular directions of the stratum C's separated surfaces deviate as much from those of these of the stratum B as these directions of its surfaces of separation do from the same directions of the separated surfaces of the stratum A. Hence the angular direction *a b q* 3 is a curved line, the ratio of whose deviation from the line *b o* increases opposite every stratum above the stratum A; and the angular direction, *a e k* 2, of the upper side of the rent is a curved line also, which deviates from the line *e n* with an increasing ratio, but one not so great as that of the under side of the rent; the width of this rent, therefore, increases upwards. In this manner the lower half of the rent, *n w v*, fig. 1, Plate XXIX., was formed; and in a similar manner, with the following exception, its upper half was formed. But as in the upper half, the ratio of contraction on the under side being greater, and on the upper side less, than the general ratio of contraction on similar sides of the lower half of the rent, the sectional lines which represent the angular direction of its upper half present a similarly disposed curve as its lower half, but it has a reverse direction; and the width of the upper half decreases from its maximum at the middle to a point at the highest extremity of the rent.

As the part of any one of the strata on the under side of this rent below the stratum A has raised that which lies upon it as much as it was itself raised by that on which it lies, with the additional distance of what it contracted less than that produced by the general ratio of contraction for a distance equal to its thickness; and as its correspondent part on the upper side of the rent has let the stratum immediately above it decend as much as its own descent below the general line of the stratum, with the additional distance of what it contracted more than the strata in general; it follows that the distances between the surfaces of separation of these strata are the respective measures of the *matter's unequal contraction* between them and the lowest extremity of the rent. Thus the distance, *a b*, between these surfaces of the stratum A is the measure of the unequal contraction of the matter between that stratum and the lowest extremity, *v*, of the rent. The distances *p q*, *r s*, *t u*, between separated surfaces of the strata F, G, H, are the measures of the matter's unequal contraction between these strata and the rent's lowest extremity respectively; and as much as the distance between the surfaces of separation of any one of these strata and the point *v* is less than the distance between the separated surfaces of the stratum A, so much is the distance of this stratum from the point *v* less than that of the stratum A. Again, as the part of any one of the strata which are situated above the stratum A on the under side of this rent has raised the stratum lying upon it as much as it was itself raised by that which lies below it, with the deduction of the distance of what its ratio of contraction is more than the general ratio; and as its correspondent part on the upper side of the rent has permitted the stratum immediately above it to descend a distance equal to that which itself descended below the general line of the stratum, with the deduction of what its ratio of contraction is less than the given ratio; it follows that the distance between the surfaces of separation of any one of these strata is the measure of the unequal contraction between that stratum and the bottom of the rent; and the difference of the distance between its surfaces of separation and that between the separated surfaces of the stratum A, is the measure of the decrease in the amount of the unequal contraction between that stratum and the stratum A: the first of which differences increases, and the second decreases as the distance of each from the stratum A increases, but both cease with the rent. The power, therefore, of the unequal perpendicular contraction of the matter to displace the strata commences at the rent's lowest extremity, accumulates in effect from that place to the middle, but from thence decreases upwards, and ceases with the rent.

As the lines, *n f v* and *v z n*, which represent the lines of distinction between the parts of the strata that have the regular position, and those which are bent towards the rent, are widest in the middle, and meet at the highest and lowest points of the rent, so the distances from the rent to which the strata are bent are greatest opposite its centre, and decrease gradually to its highest and lowest extremities. Again, as the sphere of the horizontal contraction has extended as far as the strata are bent, and as the distance from the rent to where the contraction in this direction commenced, is greatest in the middle of the rent, and decreases upwards and downwards from this place; so the distance which the strata have contracted horizontally is greatest at the middle, and decreases gradually towards the highest and lowest extremities of the rent.

From what I have seen of the smaller rents of this shape, in general when the distance *f w* is four yards, the distance *d c* is two feet, the distance *v b* 90 yards, and the distance *a b* three yards. From these data the contraction horizontally is one-sixth of the distance in which that contraction took place. How much the contraction is perpendicularly we have no direct means of ascertaining; but that it is as much in this as in the horizontal direction is extremely probable; and on this supposition the distance which the strata have contracted less on the under than on the upper side of this rent is one-fifth of their whole contraction in this direction.

The matter near this rent has contracted more horizontally in one part than in another. Thus its horizontal contraction is greater opposite the part *b*, fig. 3, Plate XXIX., than opposite the part *c*; and more opposite the part *d* than the part *e*; and so on. The inequality in the matter's contraction in this direction is from one-fifth to one-sixth of its whole contraction. This inequality gives the curvilinear bends to *both* sides of a rent, which necessarily produce inequalities in its width. But that arrangement of these inequalities represented by fig. 3 was formed by the inequalities of the matter's contraction taken perpendicularly as well as horizontally; because similar bends in the sides of this rent are never *wholly* opposite each other, but always situated lower on the upper side than on the under side; and this arrangement is owing to the circumstance before mentioned, that the parts of the strata on the former side, or the side *g m*, have contracted more perpendicularly or sunk lower than on the latter side, the side *a f*, of the rent. When the hollow parts on the upper side of a rent have sunk directly opposite similar hollows on the rent's under side, the variation in the width before described and represented by fig. 4, Plate XXIX., is produced; but corresponding hollows are not opposite each other; for the hollow *a* was originally opposite the hollow *a* 1, &c. There are many other variations in the ratio of the strata's bending, and in the regularity of the widths and positions of bended-tabular rents, which depend upon certain modifications of the principles already laid down. Some of these variations I will afterwards describe in separate essays.

ARTICLE II.

Experiments to determine the Definite Proportions in which the Elements of Organic Nature are combined. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

(*Continued from* p. 409, Vol. IV.)

III. *Analysis of the Ternary Oxides.*

1. *Analysis of citric acid.*—I shall begin with citric acid, because its composition is the most simple of all the ternary oxides. Ten parts of citrate of lead, strongly dried and decomposed by sulphuric acid, produced 8·9346 parts of sulphate of lead, equivalent to 6·582 of oxide of lead. Hence it follows that citrate of lead is composed of

Citric acid	34·18	100
Oxide of lead	65·82	190
	100·00	

These 190 parts of oxide of lead contain 13·588 parts of oxygen. In analyses made by combustion I found only 187 or 188 parts of oxide of lead combined with 100 acid. Citrate of lead retains humidity very strongly, and hence analysis makes the base always too small. As this analysis was one of the first that I made, I had not at that time found out the method of drying the substances in a sand-bath heated and exposed in a vacuum.

I made a great many experiments on the citric acid, because this acid and the tartaric served as a kind of exercise to prepare me for these experiments. A part of them was unsuccessful, because I had not at that time acquired the necessary practice in analysis. One among the successful experiments gave as the result of the analysis of 1 part of citrate of lead (which by combustion left for residue 64·956 per cent. of oxide of lead, and in which there was of course 35·044 per cent. of acid,) 0·1145 water, and 0·503 of carbonic acid in the state of gas; to this if we add 0·03 for the carbonic acid retained by the soda, we obtain 0·533 of carbonic acid. This shows us that citric acid contains per cent.:

Hydrogen	3·800
Carbon	41·369
Oxygen	54·831
	100·000

We have seen that the capacity of saturation of this acid is 13·585; but 13·585 × 4 = 54·34. This shows us that citric acid ought to contain four times as much oxygen as the base by which it is neutralized. When we turn these numbers into volumes, we find that they agree perfectly with the idea that citric acid is composed of equal volumes of its elements; that is to say, it ought to contain per cent.:

Hydrogen	3·634
Carbon	41·270
Oxygen	55·096
	100·000

If this be so, citric acid has the simplest composition of any of the ternary oxides. Its capacity of saturation indicates that it ought to contain four volumes of oxygen, four volumes of carbon, and four of hydrogen. Must we consider it as a compound of 12 atoms or of three? Experiments on the combination of citric acid with water seem to prove that it contains only three atoms, an atom of each constituent.

When we compare the result of my former experiments on the quantity of water contained in citric acid, we find that they do not coincide with the result of the analysis of citrate of lead. 10 parts of citric acid in crystals dissolved in water, neutralized by ammonia, and mixed with nitrate of lead, as long as any precipitate fell, produced 23·756 of dry citrate of lead. In this citrate there is 8·3 of citric acid; that is to say, that 100 parts of acid contain 17 of water: or that 100 parts of acid combine with 20·5 of water, which contain 18·1 oxygen. But 18·1 × 3 = 54·3; that is to say, that in crystallized citric acid the acid contains three times as much oxygen as the water.

I took five parts of citric acid reduced to a coarse powder, and exposed them in a glass capsule exactly weighed to a temperature between 118° and 122°. In 24 hours it lost 0·43 of its weight, and had assumed the form of a white light powder. In another experiment the loss was 0·424. No further loss was sustained by longer exposure to heat. This experiment shows that citric acid by efflorescing loses from 8·58 to 8·6 per cent. of its weight of water, which is obviously the half of the water of crystallization contained in the acid. Citric acid does not part with the rest of its water; for if we heat it to a certain temperature, it loses indeed weight, but at the same time it becomes brown, is decomposed, and leaves a brownish deliquescent mass, no longer possessing the properties of citric acid. In order to expel the water we must substitute another oxide in its place. From these experiments it follows, that in effloresced citric acid, or *citrate of water*, as it may be called, the acid contains six times as much oxygen as the water, and that in crystallized *citrate of water* the acid contains three times as much as the water.

This property of citric acid to allow itself to be saturated by water in another proportion than by the stronger oxides is peculiar; but it is easily explained by the simplicity of the composition of citric acid, and cannot take place in other circumstances. The composition of citric acid may then be expressed by this formula, $H + C + O$.

My expectation of being able to illustrate the composition of this acid by the examination of a subcitrate of lead was disappointed, by the property which the neutral citrate has of dissolving in ammonia, and of forming a triple salt, from which the ammonia could not be driven off even in a vacuum.

Thenard and Gay-Lussac, in their experiments on citric acid, obtained as a result 6·33 hydrogen, 33·811 carbon, 59·859 oxygen. This result differs very much from mine. Their analysis of the citrate of lime employed by them shows that very little water of combination remained in the salt. Besides, a correction for the water of combination remaining in the salt would only increase the difference between our results. I am unable to divine the cause of this difference.

2. *Tartaric acid.*—100 parts of tartrate of lead analysed by combustion, left 62·5 of oxide of lead in one experiment, and 62·48 in another. This differs but little from my old experiments, in which I found 62·2 per cent. of oxide of lead in this salt. It is composed then of

Tartaric acid	37·5	100
Oxide of lead	62·5	167
	100·0	

But 167 of oxide of lead contain 11·94 of oxygen. In one of my old experiments I found that 100 of tartaric acid are neutralized by 70·4 of potash, in which there are 11·93 oxygen. I have shown likewise in these experiments, that tartaric acid must contain five times as much oxygen as the base by which it is saturated.

$1\frac{1}{3}$ of tartrate of lead, equivalent to 0·5 of tartaric acid, produced in different experiments from 0·161 to 0·162 of water, equivalent to from 3·79 to 3·807 per cent. of the weight of the acid of hydrogen. In the same experiments I obtained from 0·654 to 0·661 of carbonic acid, equivalent to from 35·36 to 35·98 per cent of the weight of the acid of carbon. The acid is then composed of

Hydrogen	3·807
Carbon	35·980
Oxygen	60·213
	100·000

But $11·94 \times 5 = 59·7$. If we examine the ratio of these numbers, we find that the hydrogen is to the oxygen :: 66 : 1000; that is to say, that there is a volume of each. The carbon constitutes $\frac{4}{5}$ of a volume. But if the oxygen is in reality five volumes, it is combined with five volumes of hydrogen and four volumes of carbon. Supposing then tartaric acid a compound of $5\,H + 4\,C + 5\,O$, calculation gives its composition as follows:—

Hydrogen	3·951
Carbon	36·167
Oxygen	59·882
	100·000

The true capacity of saturation of this acid is then 11·976, instead of 11·94 given by experiment. It follows from this, that in the neutral tartrates the carbon of the acid is combined with 1½ volume of oxygen, including the oxygen of the base. I have not been able to obtain a subtartrate of lead.

Let us now examine the result of the analysis of this acid made by Thenard and Gay-Lussac. They found it composed of hydrogen 6·629, carbon 24·05, oxygen 69·321; but they employed tartrate of lime, in which they supposed 77·577 of tartaric acid. That is to say, that they paid no attention to the water of combination of the citrate of lime. To find the quantity of this water I made the following experiments.

I dissolved neutral tartrate of potash in water, and precipitated it by muriate of lime; the neutrality was not altered. Hence it follows, that 100 tartaric acid saturate a quantity of lime containing 11·976 of oxygen; that is to say, equal to that in the potash separated from the tartaric acid. I burnt 100 parts of this tartrate of lime in a platinum crucible, till the lime was reduced to a caustic state. To be quite sure of getting rid of all the carbonic acid, I allowed the lime to unite with a little water, and exposed it again to a strong heat; its weight was not altered. It weighed 21·64 parts, and dissolved in diluted muriatic acid without the smallest disengagement of gas. 21·64 parts of lime require for saturation 50·55 of tartaric acid; hence it follows, that tartrate of lime is composed of

Tartaric acid	50·55
Lime	21·64
Water of combination	27·81
	100·00

But 21·64 of lime contain 6·094 of oxygen, and 27·81 of water, 24·54. Now 6·094 × 4 = 24·376; that is to say, that the salt contains a quantity of water of combination, the oxygen in which is four times that in the lime. It follows from this, that what Thenard and Gay-Lussac considered as 100 tartaric acid, was in fact a mixture of 65·46 of acid with 34·54 of water. If we subtract this proportion of water from their result, we obtain tartaric acid composed of

Hydrogen	3·912
Carbon	36·888
Oxygen	59·200
	100·000

which agrees perfectly with the result of my experiments.

3. *Oxalic acid.*—My former analytical experiments on the oxalate of lead, indicate 25·2 of oxalic acid in 100 of oxalate of lead. The analysis was made by combustion, but without attending to the lead which was reduced and concealed in the oxide of lead obtained. Later experiments on four parts of oxalate of lead gave me a loss amounting to 0·9835; the oxide being dissolved in vinegar, 0·095 of metallic lead remained. This indicates 0·009 of oxygen, which must be subtracted from the loss in order to obtain the true quantity of acid; of course it amounted to 0·9816, or 24·54 per cent. of the weight of the oxalate. This result was constant in various experiments. The oxalate of lead then is composed of

Oxalic acid	24·54	100
Oxide of lead	75·46	307·5
	100·00	

But 307·5 of oxalate of lead contain 22·062 of oxygen. And since according to my old experiments on oxalic acid, it contains three times as much oxygen as the base, it follows that it ought to contain 66·186 per cent.

I mentioned before that the analyses which I made by means of the brown oxide of lead gave me too little oxygen and too much hydrogen and carbon. The result of one of these experiments on oxalic acid was 0·66 per cent of hydrogen, 34·34 of carbon, and 64·99 of oxygen.

In my repetitions by my new method, I burnt four parts of oxalate of lead with two parts of hyperoxymuriate of potash. The quantity of water produced weighed only 0·019; hence I suspected that this oxalate, which had been strongly heated, might have been partly decomposed. I therefore repeated the experiment with an oxalate that had been dried in a temperature below 212°. I obtained exactly the same result. I next suspected that I had taken too little hyperoxymuriate of potash, in consequence of which some empyreumatic oil might have been formed, though no trace of it appeared in the water. I therefore burnt two parts of oxalate of lead with two parts of of hyperoxymuriate. I obtained 0·004 of water in the receiver, and 0·001 in the tube with the muriate of lime. So that the result was almost exactly the same, giving 0·02 instead of 0·019 of water from four of oxalate. These four parts contain 0·9816 of oxalic acid; hence it follows, that this acid contains only 0·23, or 0·24 per cent. of hydrogen.

The carbonic acid produced weighed from 1·138 to 1·140, which makes 32·16 per cent. of carbon. A little of the carbonic acid remained, combined with the soda as in other experiments; but this quantity could not be determined, because a portion of the oxide of lead was converted into minium, and gave a red colour to the burnt mass. The colour was the same when I endeavoured to keep the whole tube red hot during the combustion of the oxalate. Another portion of the oxide was changed into submuriate, as I satisfied myself by digesting the oxide in nitric acid, after it had been well washed. Oxymuriatic acid was disengaged, while at the same time neutral nitrate and muriate of lead were formed.

Though in consequence of this circumstance, it is impossible to determine the quantity of carbon with perfect precision, we know at least that it must contain more than 32·6, which would have been the result if all the oxide had been converted into minium, and less than 35 per cent., which would have been the result if no part of the oxide had been converted into minium. By attending to these two extremes, we find without difficulty, that the oxygen and carbon exist in such proportions in oxalic acid, that there is one volume of carbon for every 1¼ volume of oxygen, or two volumes of carbon for every three volumes of oxygen.

To determine the composition of oxalic acid, we must then ascertain the relation between the volumes of its oxygen and hydrogen. This is difficult, because the quantity of hydrogen is so small, that analysis is not able to decide concerning it. I think, however, that we can find it from other circumstances. There can be no doubt that the composition of oxalic acid is such, that in the oxalates the hydrogen of the acid is combined with a certain number of entire volumes of the radicle of the base; but saline bases may contain one, two, or three volumes of oxygen, without this producing any change in the ratio of the oxygen of the acid with that in the base. When we reflect on this, we perceive that with the known ratio between the carbon and oxygen in oxalic acid, there are only three modes of combination in which the above stated circumstance, (that a volume of hydrogen in the acid is combined with a certain number of entire volumes of the radicle of the base,) can take place; namely, $H + 2\,C + 3\,O$, or $H + 12\,C + 18\,O$, or $H + 24\,C + 36\,O$. For the degrees above this are not probable. The difference between these three degrees is so great, that the quantity of hydrogen found by experiment must be sufficiently exact to determine which of them belongs to oxalic acid. But analysis has given much more hydrogen than is consistent with the third of these, but less than would be requisite for the first mode of combination. But all the circumstances of the analysis agree perfectly with the idea, that oxalic acid is composed of one volume hydrogen, 12 volumes carbon, and 18 volumes oxygen.* This constitutes per cent.:

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* In my memoir On the cause of Chemical Proportions, *Annals of Philosophy*, December, 1813, I have stated by a mistake in the calculation that this acid contains 27 volumes of carbon. It is obvious that this error is of no consequence relative to the subject treated of in that memoir. I acknowledge however that at present I do not lay much stress on that argument against the corpuscular doctrine. For it is clear that the compound atoms of organic nature ought to have a mechanical structure as different from that of inorganic atoms as their composition is different.

Hydrogen	0·244
Carbon	33·222
Oxygen	66·534
	100·000

It follows from these reflections, that in an oxalate whose base contains only a single volume of oxygen, the hydrogen of the acid is combined with six volumes of the radicle of the base. When on the other side the base contains two volumes of oxygen, the hydrogen of the acid is combined with three volumes of the radicle of the base. When the base contains three volumes of oxygen, then the hydrogen is combined with two volumes of radicle. This may be seen in the following formulas, representing the oxalate of copper, the oxalate of lead, and the peroxalate of iron.

$$6\,\mathrm{Cu\,O} + \mathrm{H\,C^{12}\,O^{18}}$$
$$3\,\mathrm{Pl\,O^{2}} + \mathrm{H\,C^{12}\,O^{18}}$$
$$2\,\mathrm{Fe\,O^{3}} + \mathrm{H\,C^{12}\,O^{18}}$$

Let us now examine the result of the analysis of this acid by Gay-Lussac and Thenard. They employed in it oxalate of lime dried in the temperature of boiling water. This oxalate in that state contains water of combination, the quantity of which determined both by the experiments of Mr. Vogel of Bayreuth, and by calculation, is such, that it contains a quantity of oxygen equal to that in the lime; that is, very nearly 12 per cent. Therefore the oxalic acid used in their experiments consisted of 80 pure acid + 20 water. If we take away these 20 of water, that is to say, 2·35 hydrogen, and 17·75 oxygen, from the result af the analysis of Gay-Lussac and Thenard, we have

	Result of Gay-Lussac and Thenard.	Do. corrected.
Hydrogen	2·745	0·493
Carbon	26·566	33·217
Oxygen	70·689	66·290
	100·000	100·000

This result agrees so well with my experiments, that it serves to confirm what I have advanced above.*

4. *Succinic acid.*—I purified succinic acid from the empyreu-

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* Dr. Thomson in the Philosophical Transactions for 1807, has given an analysis of oxalic acid made by dry distillation and the analysis of the products. He found it composed of

Hydrogen	4
Carbon	32
Oxygen	64
	100

I am astonished that he could have come so near the truth notwithstanding the difficulties of the method which he followed.

matic matter with which it is usually impregnated, in the following manner. I sublimed it in a moderate heat, the product of the sublimation is succinic acid, contaminated with colourless oil of amber, which becomes coloured by exposure to the air. I combined it with carbonate of ammonia, adding a slight excess of the alkali. I then digested it with charcoal, prepared by exposing coagulated blood to a strong heat in a covered crucible. This charcoal absorbed the empyreumatic matters, and allowed the succinate of ammonia to crystallize of a white colour. From this succinate of ammonia, I prepared by the addition of nitrate of lead. with the precautions already stated, pure succinate of lead. 100 parts of this succinate analysed by combustion gave 69·1 of oxide of lead. Hence the salt is composed of

Succinic acid	30·9	100·00
Oxide of Lead	69·1	223·62
	100·0	

But 223·62 of oxide of lead contain 16 of oxygen.

Subsuccinate of lead obtained by digesting the neutral succinate in ammonia, dried in a vacuum at the temperature of boiling water, and analyzed by combustion, yielded 86·93 per cent. of oxide of lead. A great excess of ammonia poured upon this subsuccinate did not alter it; for it gave afterwards the same analytical result. The subsuccinate of lead then is composed of

Succinic acid	13·07	100
Oxide of lead	86·93	666
	100·00	

But 223·64 × 3 = 670·8, which differs but little from 666. Hence it is evident, that the succinic acid in this salt is combined with three times as much base as in the neutral succinate. Of course succinic acid must contain three volumes of oxygen.

1·294 of succinate of lead, equivalent to 0·4 of succinic acid, produced by combustion 0·1536 of water, equivalent to 4·512 per cent. of hydrogen, and 0·656 of carbonic acid in form of gas, to which adding the 0·044 remaining in the soda, the whole amounts to 0·7, equivalent to 47·6 per cent. of carbon. The acid therefore is composed of

Hydrogen	4·512
Carbon	47·600
Oxygen	47·888
	100·000

But 16 × 3 = 48. Hence it follows, that the acid contains three volumes of oxygen. We find that the carbon and hydrogen amount each to four volumes; so that succinic acid is composed of 4 H + 4 C + 3 O, which makes in a hundred parts

Hydrogen	4·218
Carbon	47·859
Oxygen	47·923
	100

The true capacity of saturation of the acid then is 15.9743, instead of 16 given by experiment.

(*To be continued.*)

ARTICLE III.

A Memoir on Iodine. By M. Gay-Lussac.*

(Read to the French Institute on the 1st of August, 1814.)

I HAVE already, in the sittings of the 6th and 20th of December, 1813, read to the Class the result of my researches† on the substance discovered by M. Courtois, to which I proposed to give the name of *iode*, on account of the beautiful violet colour of its vapour. To these researches, the object of which was to determine the nature of *iodine*, and the situation in which it ought to be placed with respect to other bodies, I have added new experiments, which I have now the honour to communicate to the Class. If there be no occasion to apologize for the length of time which has elapsed since I read my first experiments, I must at least beg your indulgence for the details into which I am about to enter. I regret that my labours have lost part of their interest, by appearing after those of M. Vauquelin, with which I was unaccquainted when I drew up my memoir. But it will be at least a satisfaction to me, if, in the subjects which we have treated in common, I have in general drawn the same conclusions as he has done.

Properties of Iodine.

Iodine when in a solid state is greyish black; but its vapour has a fine violet colour. Its odour is exactly that of chlorine, weakened. It is often in scales similar to those of micaceous iron ore, sometimes in rhomboidal plates, very large and very brilliant. I have obtained it in elongated octohedrons about a centimetre in length. Its fracture is lamellated, and it has a fatty lustre. It is very soft and friable, and may be rubbed to a fine powder. Its taste is very acrid, though it be but very little soluble in water. It gives a deep brown stain to the skin, but this colour is soon removed. Like chlorine it destroys vegetable colours; but with much less energy. Water dissolves about one seven thousandth part of its weight of iodine, and acquires an orange yellow colour. The specific gravity of iodine at 62½° is 4·948. It melts at 225°, and is volatilized under the common pressure of the atmosphere, at the temperature of 347° or 356°. I ascertained this last temperature by putting iodine in excess in concentrated sulphuric acid, which has little action on it, and observing at what degree of heat its vapours passed up through the acid, I fixed the boiling point at 347° and 356°, because these two numbers are the result of two experiments made in circumstances a little different from each other.* As iodine, when mixed with water, may be distilled over with that liquid, it was at first concluded that its boiling point was nearly the same with that of water; but this is a mistake. At the temperature of 212°, the vapour of iodine mixes with the vapour of water in considerable quantity, and is carried with it to the receiver where it condenses. Iodine may even be distilled at a much lower temperature. The same thing happens to the essential oils; though they boil only at about 309°, yet they may be distilled over at 212° when mixed with water. Iodine seems to be a non-conductor of electricity. When a small fragment of it is put into the galvanic circuit, the decomposition of water immediately stops.

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* Translaed from the Annals de Chimie, vol. xci. p. 5. July 1814.

† Printed in the *Annals of Philosophy*, iii. 106.

Iodine is not combustible. It cannot even be combined directly with oxygen. I consider it as a simple substance, and place it, in consequence of the experiments which I have made, between sulphur and chlorine; because its affinities are stronger than the former and weaker than the latter of these bodies. Like them it forms two acids, one by combining with hydrogen, and another by combining with oxygen; and most of its combinations have considerable analogy with those formed by these two bodies. The compounds of chlorine, iodine, and sulphur with hydrogen, possessing the properties of acids formed by oxygen, ought to be comprehended in the same class, and under the same acid name. In order to distinguish them I propose to prefix to the specific name of the acid, which we consider the generic term, *hydro*; so that the acid combinations of chlorine, iodine, and sulphur with hydrogen, shall have the names of *hydrochloric acid*, *hydriodic acid*, and *hydrosulphuric acid*. The acid compounds of oxygen with the same bodies, may receive, according to the principles of the nomenclature adopted, the names of *chloric*, *iodic*, *sulphuric acids*. The terms *chlorurets* and *iodurets* may denote the compounds of chlorine and iodine with combustibles or oxides. Thus the *oxymuriate of lime* will be called *chloruret of lime*.

On the Combination of Iodine with simple Substances, and in particular of Hydriodic Acid.

Iodine combines with most combustibles; but I have only examined a few of these combinations.

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* These experiments are somewhat hazardous. Though I had put pieces of glass into the sulphuric acid, the iodine in the second experiment was converted all at once into vapour and drove out the hot acid which burnt my right hand and foot severely.

Phosphorus unites to iodine in different proportions, with the disengagement of heat, but of no light. One part of phosphorus and eight parts of iodine formed a compound of a red orange brown colour, fusible at about 212°, and volatile at a higher temperature. When brought in contact with water, phosphureted hydrogen gas is disengaged, flocks of phosphorus are precipitated, and the water which is colourless contains in solution phosphorous and hydriodic acids.

One part of phosphorus and 16 of iodine, produce a matter of a greyish black colour, crystallized, fusible at 84°. The hydriodic acid produced by bringing it in contact with water, is colourless, and no phosphureted hydrogen gas is disengaged.

One part of phosphorus and 24 of iodine produced a black substance, partially fusible at 115°. Water dissolves it, producing a strong heat, and the solution had a very deep brown colour, which was not removed by keeping it for some time in a gentle heat. The proportion of one phosphorus and 16 iodine, results from the ratio between phosphorus and iodine, on the supposition that the phosphorus is converted into phosphorous acid. One phosphorus and 24 iodine should have given me colourless hydriodic acid, on the supposition that the phosphorus was converted into phosphoric acid.* I had indeed ascertained that phosphorous acid converted iodine into hydriodic acid; but the effect stops or becomes very slow, before all the phosphorous acid is destroyed. This is the reason, that with the proportion one phosphorus and 24 iodine, we obtain a deep coloured acid. It holds a considerable quantity of iodine in solution. We see likewise, that if there form only phosphorous acid, when the phosphorus is in excess, there is formed on the contrary phosphoric acid, whenever the iodine is more than 16 times greater than the phosphorus.

With 1 phosphorus and 4 iodine we obtain two compounds very different from each other. One of them has the same colour as that formed of one phosphorus + 8 iodine, and seems to be the same with it. It melts at 217¼°, and when dissolved in water gives a colourless hydriodic acid, phosphureted hydrogen, and phosphorus, which precipitates in orange yellow flocks. The other compound is reddish brown, does not melt at 212°, nor at a considerably higher temperature. Water has no sensible action on it. Potash dissolves it with the disengagement of phosphureted hydrogen gas, and when chlorine is poured into the solution it shows only traces of iodine. When heated in the open air, it takes fire, and burns like phosphorus, emitting white vapours without any iodine. I condensed these vapours in a moistened glass jar, and I could not perceive any iodine among them. This red substance is always obtained when the phosphorus is in the proportion of one to four of iodine. I am induced to consider it as identical with that red matter which phosphorus so often furnishes, and which is at present considered an oxide. But I have not compared the two bodies, as I had no oxide of phosphorus in my possession. Very little oxygen seems to be necessary to convert phosphorus into this red matter; and as the phosphorus which I employed, though well wiped, was not quite free from humidity, the formation of this substance during my experiment does not present any difficulty. It is a subject of investigation which deserves to fix the attention of chemists.

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* I have supposed in determining these proportions, that 100 of phosphorus combine with 100 oxygen to form phosphorous acid, and with 150 to form phosphoric acid.

In whatever proportions the ioduret of phosphorus has been made, it exhales, as soon as it is moistened, acid vapours, owing to the hydriodic acid formed by the decomposition of the water. To obtain this gas without any mixture of phosphureted hydrogen, it is necessary to employ an ioduret in which the phosphorus at most does not exceed a ninth part. The method which I employ is to put the ioduret into a small crucible, and to moisten it with a little water, or what is better, with water already impregnated with hydrionic acid. We may employ likewise a small bent tube. We begin by putting the iodine into this tube; then reversing it over mercury, we drive out the air which it contains, by introducing a glass rod which almost fills it. The phosphorus is then brought in contact with the iodine, by introducing it through the mercury. As soon as the contact takes place, the two substances combine, and we collect the gas which is disengaged, by putting the extremity of the tube under a glass jar standing in the mercury.*

As soon as the hydriodic gas comes in contact with mercury, it is decomposed. The surface of the metal is covered with a greenish yellow substance, which is an ioduret of mercury; and if the contact is prolonged for a sufficient time, or if we agitate, the hydriodic gas is completely decomposed. The iodine combines completely with the metal, and there remains a volume of hydrogen gas, which is exactly one half of that of the hydriodic gas. I have made this gas pass upon zinc and potassium, and the result has always been hydrogen gas and an ioduret. From this analysis, and the phenomena which iodine exhibits with hydrosulphuric acid, (*sulphureted hydrogen*), and those of the ioduret of phosphorus with water, there can remain no doubt about the nature of hydriodic gas.

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* When distilling a mixture of hydriodic acid and an ioduret of phosphorus, (made without any determined proportions,) in order to obtain the hydriodic acid, towards the end of the operation cubic crystals were formed in the neck of the retort; they were white, and as transparent as wax. On hot coals they burnt like phosphorus. When put into water, they were instantly decomposed, with a copious discharge of subphosphureted hydrogen gas, and allowing flocks of phosphorus to precipitate. The water contained hydriodic acid. Concentrated sulphuric acid, into which these crystals were plunged, became brown, as happens when it decomposes hydriodic acid. But it soon became reddish yellow and milky, probably owing to the phosphorus which precipitated. I had too few of these crystals to subject them to more numerous trials and determine exactly their nature. I was led to consider them as composed of hydriodic acid and phosphorus, and attempted to obtain them by passing a current of hydriodic acid gas over phosphorus. I obtained the new compound, but its quantity did not correspond with that of the phosphorus and hydriodic acid. Further researches are necessary.

This gas is colourless. Its odour is similar to that of hydrochloric gas, (*muriatic acid.*) Its taste is very acid. It contains half its bulk of hydrogen, and saturates its own bulk of ammoniacal gas. Chlorine instantly deprives it of its hydrogen. A beautiful violet vapour is produced, and hydrochloric gas is formed. I took its specific gravity by weighing it in a glass vessel the capacity of which I knew, weighing it in succession full of air and of gas.[*] I found by this method in a first experiment, that it is 4·602 times heavier than common air; and in a second experiment, more exact than the first, that its specific gravity is 4·443. This specific gravity ought to be a little too great, because notwithstanding the precaution which I took to pass the gas through a glass tube cooled down to — 4°, slight traces of moisture were seen in the weighing bottle. These were more conspicuous in the first experiment than in the second. When we compare hydriodic gas with hydrochloric, we obtain a density differing but little from my last experiment, and which ought to be the true one.

I must put the reader in mind in the first place, that from the experiments of M. Thenard and myself, one volume of chlorine by combining with one volume of hydrogen produces exactly two volumes of hydrochloric gas; hence it follows, that the density of this last gas is the mean of that of chlorine and hydrogen. But since a volume of chlorine takes one volume of hydrogen, its ratio to oxygen in bulk will be that of two to one, and its ratio in weight may be immediately deduced from this. We do not know the density of the vapour of iodine; but from experiments to be stated below, I have found that the ratio of oxygen to iodine is 1 to 15·621. Now the density of a demivolume of oxygen being 0·55179, $0·55179 \times 15·621 = 8·6195$ will represent the density of iodine under the volume taken for unity. If to this density we add that of hydrogen, 0·07321, and take half the sum, we have 4·4288 for the density of hydriodic gas; and it is composed by weight of 100 iodine and 0·849 hydrogen. The density of the vapour of iodine is the greatest of all those with which we are acquainted. It is 117·71 greater than that of hydrogen; and as it is according to the volume of their vapours that bodies combine, this explains why iodine does not take the hundredth of its weight of hydrogen to be converted into an acid. With a denser vapour, as would be the case with that of mercury, the proportion of hydrogen requisite would be still smaller, as in the combination of mercury

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[*] Let p be the weight of the vessel full of air; P its weight full of water: for a first approximation $P - p$ will express the volume of water which it contains. If δ represent the ratio between the density of air and that of water under a given pressure and temperature, $P - p + (P - p)\,\delta$ will be a second expression of the capacity of the vessel more exact than the first, and which in ordinary cases is sufficient. Greater accuracy may be attained by the formula:—

$$P - p + (P - p)\,\delta + (P - p)\,\delta^2 + \&c.$$

with hydrogen and ammonia. We must not then reject the influence of a very small quantity of matter in a compound, unless it be demonstrated that the density of its vapour bears a considerable ratio to that of the other elements of the compound, or, which comes to the same thing, unless it be demonstrated that its capacity of saturation is small.

Hydriodic gas is partly decomposed at a red heat. The decomposition is complete if it be mixed with oxygen. Water is formed and iodine separated. I find on the other hand, that when iodine and vapour of water are passed through a red-hot porcelain tube, no decomposition takes place. This constitutes the great difference between iodine and chlorine; for this last substance takes hydrogen from oxygen. But the circumstance shows a nearer relation between iodine and sulphur, as oxygen takes hydrogen from both.

Hydriodic gas is very soluble in water. It gives it a great density, and renders it smoking when it is dissolved in great quantity. The most convenient way of obtaining liquid hydriodic acid is to dissolve ioduret of phosphorus in water. It is separated from phosphorous acid by heat. Hydriodic acid is prepared with still greater facility, by putting iodine in water and making a current of hydrosulphuric gas pass through the liquid. This gas gives out its hydrogen to the iodine, and allows its sulphur to precipitate. The liquid is heated to drive off the excess of hydrosulphuric acid. It is then filtered, or the liquid is decanted off the precipitated sulphur. By this method a colourless hydriodic acid is obtained. The acid prepared by these means is not very concentrated, unless care has been taken, in distilling that prepared by phosphorus, not to mix the last distilled portions with the first, which consist of little else than pure water. This acid, like sulphuric, may be concentrated by evaporation. While its temperature is lower than 257°, the liquid that distils over may be thrown away, because it contains very little acid. Above this point the acid itself begins to distil over, and the temperature remains constant at 262½°. The specific gravity of the acid is then 1·7, and it does not sensibly vary. This property of hydriodic acid to boil only at 262½°, renders it a powerful acid, and prevents it from being disengaged from its combinations by the volatile acids.

Hydriodic acid always becomes more or less coloured when distilled. It even becomes coloured at the ordinary temperature, provided it be in contact of the air. Oxygen is absorbed, water formed, and the iodine, instead of precipitating, dissolves in the acid, and gives it a brownish red colour, the deepness of which is proportional to the quantity of iodine held in solution. I have attempted, but in vain, to destroy this colour by boiling the acid; hence I conclude, that iodine has a great affinity for hydriodic acid: for if its volatility were not diminished by the combination, it ought to be disengaged with the vapour of the water. However I do not think that we ought to consider this coloured acid as a peculiar acid.

Concentrated sulphuric acid, nitric acid, and chlorine, immediately decompose hydriodic acid. They seize upon its hydrogen, and the iodine precipitates, or exhales in purple vapours. Chlorine is one of the most delicate re-agents to detect small quantities of hydriodic acid; but it must be added cautiously: for when an excess is employed, it dissolves the iodine before it has time to precipitate, or at least colour the liquid. Like hydrosulphuric acid, hydriodic acid is decomposed by solutions of peroxide of iron. When heated by the oxides which give chlorine with hydrochloric acid, iodine is evolved, together with a hydriodate or an ioduret. If for example, it be heated with the black oxide of manganese, we obtain iodine and hydriodate of manganese; but with the red oxide of lead, we obtain iodine and an ioduret. Hydriodic acid forms compounds with the different bases, which have a great deal of resemblance to the hydrosulphates and the hydrochlorates.

Let us recapitulate the principal characters of hydriodic acid. In the gaseous state it is speedily decomposed by mercury, which is converted into an ioduret of a greenish yellow colour. With chlorine it immediately produces a fine purple vapour of great intensity. In the liquid state it is speedily decomposed, and coloured when exposed to the air. Concentrated sulphuric acid, nitric acid, and chlorine, separate iodine from it. Sulphureted hydrogen does not alter it in the least. When poured into a solution of lead it forms a fine orange precipitate. In the solution of peroxide of mercury it forms a red precipitate, and with silver a white precipitate insoluble in ammonia. I thought proper to give the properties of hydriodic acid in this place, because this will render more intelligible the account which I am going to give of the combinations of iodine with other bodies.

Iodine forms with sulphur a weak compound of a greyish black colour, radiated like sulphuret of antimony. Iodine is separated from it when it is distilled with water.

Hydrogen, whether dry or moist, did not seem to me to have any action on iodine at the ordinary temperature; but if, as was done by M. Clement, in an experiment in which I was present, we expose a mixture of hydrogen and iodine to a red heat in a tube, they unite together, and hydriodic acid is produced, which gives a reddish brown colour to water. We found that 100 parts of iodine absorb 1·53 of hydrogen, in order to be converted into an acid. But this proportion is a great deal too great, as I found afterwards that hydriodic acid is composed of 100 iodine and 0·849 hydrogen.

Charcoal has no action upon iodine, either at a high or low temperature. Several metals on the contrary, as zinc, iron, tin, mercury, and potassium attack it with facility, even at a low temperature, provided they be in a divided state. Though these combinations take place readily, they produce but little heat, and but rarely any light. The compound of iodine and zinc, which I call *ioduret of zinc*, is white. It melts readily, and is sublimed in the state of fine acicular four-sided prisms. It is very soluble in water, and rapidly deliquesces in the air. It dissolves in water without the evolution of any gas. The solution is slightly acid, and does not crystallize. The alkalies precipitate from it the white oxide of zinc, concentrated sulphuric acid disengages hydriodic acid and iodine, because sulphurous acid is produced. We may conceive that water dissolves the ioduret of zinc without undergoing decomposition; but as the slightest force would afterwards decompose it, and besides, as the solution has exactly the same characters as the hydriodate of zinc obtained by combining the oxide of zinc with hydriodic acid, we have the same motives for admitting that the water is decomposed during the solution of the ioduret, as for admitting that it is formed when hydriodic acid dissolves the oxide. We may, however, adopt either supposition. For the sake of greater simplicity I shall adopt the latter, in order to determine the ratio of iodine to oxygen and hydrogen.

When iodine and zinc are made to act on each other under water in vessels hermetically sealed, on the application of a slight heat the water assumes a deep reddish brown colour, because as soon as hydriodic acid is produced it dissolves iodine in abundance. But by degrees, the zinc which I suppose in excess, combines with the whole iodine, and the solution becomes colourless like water. In three experiments, which differed little from each other, and of which I have taken the mean, I found that 100 iodine combined with 26·225 zinc. But 26·225 zinc combine with 6·402 oxygen, which saturate 0·849 hydrogen; consequently, the ratio of oxygen to iodine is 6·402 to 100, or 10 to 156·21, and the ratio of hydrogen to iodine is 0·849 to 100, or 1·3268 to 156·21. Thus if we represent the number for oxygen with Dr. Wollaston by 10, the number for iodine will be 156·21; the ratio which I assigned in my first experiments, as well as that of Davy, was very inaccurate.

Iron is acted upon by iodine in the same way as zinc. The ioduret of iron is brown, and fusible at a red heat. It dissolves in water, and the colour of the solution is a light green, like that of the chloruret of iron.

Iodine and potassium combine with a great deal of heat, and with the disengagement of a light which appears violet through the vapour of iodine. The compound melts, and sublimes at a red heat. On cooling it assumes a pearly and crystalline appearance. Its solution in water is perfectly neutral. It is easy to determine the proportion of these iodurets from those of ioduret of zinc. If we attend to this circumstance, that the quantities of iodine which combine with each metal, are proportional to the quantities of oxygen with which it combines; of course 100 potassium, which requires 20·425 oxygen to convert it into potash, combine with 319·06 of iodine.

The ioduret of tin is very fusible. When in powder, its colour is a dirty orange yellow, not unlike that of glass of antimony. When put into a considerable quantity of water it is completely decomposed. Hydriodic acid is formed, which remains in solution in water, and the oxide of tin precipitates in white flocks. If the quantity of water be small, the acid being more concentrated, retains a portion of oxide of tin, and forms a silky orange-coloured salt, which may be almost entirely decomposed by water. Iodine and tin act very well on each other in water of the temperature of 212°. We may, by employing an excess of tin, obtain pure hydriodic acid, or at least an acid containing only traces of the metal. The tin must be in considerable quantity, because the oxide which precipitates on its surface diminishes much its action on iodine.

Antimony presents with iodine the same phenomena as tin; so that we might employ either for the preparation of hydriodic acid, if we were not acquainted with preferable methods.

The iodurets of lead, copper, bismuth, silver, and mercury, are insoluble in water, while the iodurets of the very oxidable metals are soluble in that liquid. If we mix a hydriodate with the metallic solutions, all the metals which do not decompose water will give precipitates, while those which decompose that liquid will give none. This at least is the case with the metals of which I have spoken: and if this fact, which I consider as general, be not a sufficient proof of the existence of hydriodates, it at least renders their existence probable.

There are two iodurets of mercury: the one yellow, the other red; both are fusible and volatile. The yellow, which corresponds to the protoxide of mercury, contains one-half less iodine than the red, which corresponds to the protoxide. In general there ought to be for each metal as many iodurets as there are degrees of oxidation.

All the iodurets are decomposed by concentrated sulphuric and nitric acids. The metal is converted into an oxide, and iodine is disengaged. They are likewise decomposed by oxygen at a red heat, if we except the iodurets of potassium, sodium, lead, and bismuth. Chlorine likewise separates iodine from all the iodurets; but iodine, on the other hand, decomposes most of the sulphurets and phosphurets.

(*To be continued.*)

ARTICLE IV.

Some Observations on the Sap of the Vine. By Dr. Prout.

About the middle of April last I was favoured by Mr. Astley Cooper with some sap which he had collected from a common white vine.* The following are a few of its properties:—

It was slightly opake, or rather had the whitish appearance of common river water. Taste sweetish, but not rough. No smell.

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* Mr. C. informs me that the vine, although it bled very profusely, seemed to produce a greater number of leaves than usual, but no grapes.

It did not affect litmus or turmeric papers in their natural states, nor the former when it was faintly reddened by acetic acid. Specific gravity not sensibly different from that of water.

1. *Potash.*—A solution of pure potash changed it to a beautiful reddish copper colour, and caused after some little time a flaky precipitate of the same colour, leaving the fluid nearly colourless and transparent. This precipitate was not redissolved by excess of potash even when heat was applied, but the application of heat changed it to a deepish brown colour. Acetic acid added in slight excess readily redissolved this precipitate.

2. *Ammonia.*—The same phenomena followed the use of this alkali as of potash above-mentioned. Acetic acid also as readily redissolved the precipitate occasioned by it as by potash.

3. *Muriatic Acid* produced no apparent change. After the addition of this acid, ammonia was added in excess, when precisely the same sort of precipitate as that above-mentioned was observed.

4. *Oxymuriatic Acid* produced no apparent change.

5. *Muriate of Barytes.*—No apparent change.

6. *Oxalate of Ammonia* produced a very sensible white precipitate.

7. *Prussiate of Potash* caused a very slight whitish precipitate.

8. *Hydro-sulphuret of Potash.*—A slight dark brown flaky precicipitate.

9. *Nitrate of Silver.*—A slight flaky precipitate, which soon became of a purple hue.

10. *Oxymuriate of Mercury.*—No apparent change.

11. *Subacetate of Lead.*—A copious yellowish-white precipitate.

12. *Infusion of Galls.*—No apparent change.

13. *Gelatine.*—No apparent change.

Four hundred and sixty grains of the sap were evaporated in a glass capsule on a sand-bath. During the evaporation air bubbles collected on the sides and bottom of the vessel. The fluid became slightly opake, and towards the end of the operation brown flocculi precipitated. There was left only ⅕ gr. of solid matter (= ·044 per cent.), about half of which was carbonate of lime, the rest a peculiar vegetable matter. This peculiar vegetable matter was not soluble in alcohol, and therefore did not agree in this respect with the ill-defined class of substances called extracts or extractives. Both it, however, as well as the lime, were evidently held in solution by some volatile acid or acids. One of these acids was doubtless the carbonic. There were also traces of the acetic acid, and likewise of an alkali (potash?), since the glass capsule on exposure to the air became sensibly moist. The quantities, however, of the last two were extremely minute.

Every thing connected with vegetable physiology is exceedingly obsure. The opinion,* however, appears to be correct, which supposes that one use of the large quantity of watery sap which flows in plants in the spring is to dissolve the thick and otherwise inactive juices which had been deposited in nearly a solid state in the vessels during the winter, and thus adapt them to the further uses required by the economy of the plant. It appears also that this solution is not effected solely by the agency of the water, which alone would perhaps be insufficient, but by the assistance of some acid or saline agent which probably pre-existed in the plant itself, and only required the presence of water to render it effective. And certainly we cannot conceive menstrua better adapted for this purpose than the above acids, not only from their considerable solvent powers, but from the ease with which they may be got rid of, either by decomposition, exhalation, &c. when they have performed their office.

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* Darwin's Phytologia, p. 28, &c. Mr. Knight, in Philosophical Transactions, 1805, p. 70, &c.

I know of no use of the large proportion of lime found in all saps; but I have some reason for believing that there is a much greater relation between this earth and the saccharine principle than has been commonly imagined.

The saps of different vegetables have been examined by Vauquelin, Chaptal, and others. There is some resemblance between the sap of the common elm as observed by Vauquelin, and the above. See Ann. de Chim. xxxi. 20; Memoires de l'Institut National, i. 288; also Dr. Thomson's Chemistry, vol. v.

Article V.

On the Use of the Cerebellum, on the Spinal Marrow, and on Respiration. By Dr. John Cross.

(To Dr. Thomson.)

SIR, *Glasgow, Nov.* 18, 1814.

Please to announce in your *Annals of Philosophy* the following discovery of the function of the cerebellum, and new theory of respiration.

From considering that the cerebrum, cerebellum, and face have in the gradual progress of animality originated and evolved together, and that organs in general have their sphere of action in their own vicinity, I was led about a year ago to conclude that as the cerebrum is the fountain of sensation and intellect, the cerebellum must be the organ which supplies with nervous energy the face and other parts of the head extrinsical of the brain, perhaps also the cerebrum itself. This opinion pressed more and more strongly on my mind until I was at length tempted to use the trephine upon living animals. Having cut out a circular piece of the occipital bone of a sheep, and laid bare the cerebellum, I applied pressure upon it with the handle of a scalpel; immediately the ears, eyes, mouth, tongue, in short the whole muscles about the head and face, became convulsed. On thrusting the handle of the scalpel through the cerebellum upon the medulla oblongata, the muscles of the body were at the same time convulsed. On pressing upon the spinal marrow as it issues from the foramen magnum, the muscles of the body were convulsed, those of the head and face remaining quiescent. The experiment has been frequently, and several times in presence of my medical friends, repeated upon sheep, rabbits, and dogs, with invariable success. This discovery led to several minute dissections of the brain and spinal marrow in dogs, cats, cows, horses, &c.; in which I found that the spinal marrow is uniformly composed of four longitudinal divisions—two larger, anterior, which may be traced into the cerebrum, and two smaller, posterior, which may be traced into the cerebellum. I suppose that the two anterior portions are the organ of feeling; the two posterior, of motion. I also found that the spinal marrow does not terminate at the sacrum in a lash of nerves, but tapers to a sharp point, to the very extremity of which I could distinctly trace the division into four portions.

On tracing the various organs of the body from man downwards through the chain of living beings, I find that there are three great fundamental functions—respiration, nutrition, and propagation, common to all organized beings. In all vegetables and animals there are neither more nor less, in so far as the living machine is concerned, than these three functions, every other being subsidiary. From this view I was induced to think more highly of respiration than as an absorber of oxygen, or an expeller of carbon. That such a large complicated organ, and such a laborious and incessant function, should serve no other purpose but drawing off carbon from the blood, is neither consistent with analogy, nor with that Divine wisdom and power which uniformly effectuates the greatest ends by the smallest means. It appears to me that vitality has been originally diffused equally over the creation; that vegetable and animal life is just a less or greater accumulation of this vitality; and that the leaves, gills, and lungs, are the chemical laboratory in which vitality is continually secreted as fast as it is expended by the vital and animal functions.

As the facts and arguments in support of this theory, and of the preceding discovery, are forthwith to come before the public, in a work to be entitled, Researches in Physiology and Physiognomy, I will not at present encroach further on your patience.

I remain, Sir, your most obedient servant,

John Cross.

Article VI.

Observations on Tanning. By Mr. Murray.

(To Dr. Thomson.)

Sir,

Noticing in the *Annals of Philosophy*, for the 1st of September, Queries respecting Tanning, I presume the liberty of submitting to you the results of experiments I have instituted on this branch of our manufactures.

Respecting the question whether *lime-water* or pure *river water* extracts the tannin from oak bark most promptly and in greater quantity, experiment enables me to decide on the *latter*.

That *lime* and *alkalies* ought to be *discarded* in the process of tanning (could we find a substitute for the former) will appear evident from immersing a piece of leather in either; the fluid will appear in a few minutes of a *blood-red colour*, and the surface of the leather *blackened*; a proof that the tannin has been absorbed by the alkaline menstruum, or lime-water.

Hence the justice of the remark, by all possible means *get rid of the lime* employed in depilation, *previous* to *immersion* in the *tannin ooze*. Lime will always produce with tannin an abundant and difficultly soluble precipitate: and for the same reason alkalies must not be used in *raising the pelt*, as they, though producing a quick and powerful extract, prevent the union of the tannin with animal gelatine by virtue of superior affinity.

The excrement of the pigeon and hen were tried with respect to their superiority over that of other animals. By sulphuric acid, a powerful effervescence takes place; and as lime had been proved to exist by the previous application of fluate and oxalate of ammonia, the *carbonic acid* was inferred to be in combination, and not the phosphoric, as the precipitate by barytes did effervesce by muriatic acid.

Nitro-muriate of tin and *gelatine* threw down an abundant *precipitate*. Hence I presume that it is to this *peculiar* substance, and not to any of the salts which it contains, we are to attribute its action on the pelt. The gelatine must be rendered less pliant by the lime used in depilation. The substance in question having an affinity for lime, would necessarily render the fibre of the hide less rigid than before, by removing the cause.

I have long suspected that *handling* did more for the beauty and value of the leather than was suspected. I endeavoured to ascertain to what this effect was owing. For this purpose a piece of leather was suspended in a vessel filled in water over the shelf of a pneumatic cistern, while the water was displaced by a stream of oxygen. The leather was in consequence beautifully *bloomed*, and this effect penetrated quite through the piece.

Diluted nitric acid stains the leather of a *blood-red*.

Sulphuric acid acts in an *inferior* degree, and makes the leather hard.

Muriatic, oxymuriatic, citric, phosphoric, acetic, and fluoric acids, communicate each a bloom. Of these the *oxymuriatic* and *muriatic acids* are by far the *preferable*.

Oxymuriatic acid and wheaten flour communicate a very fine light bloom.

It was therefore of considerable moment to ascertain whether the effect was owing to the *fecula* or *gluten* of the flour employed.

Having separated these constituents, I found *fecula simply* to communicate a bloom. *Oxymuriatic acid* and *gluten* had a *very bad effect*. Hence a self-evident proposition becomes irresistible: that those nutritious grains containing less gluten are the most proper; thus the farenaceous seeds of oats, barley, rye, &c.

The oxymuriate of lime appears to communicate a fine bloom: and as this acid may be employed to extract the lime after the process of depilation, so the salt of oxymuriate of lime may be employed afterwards to improve the bloom, so requisite to those tanners who prepare for the London market.

Two very respectable tanners have informed me that on my suggestion muriatic acid and oatmeal, and barley flour, have been used by them with great success. The muriatic acid renders hides which are required to possess this property pleasantly flexible.

"Les choses ne sont estimables qu'autant qu'elles sont utiles."

I am, with much respect,
Your very humble servant,

Stowmarket, Oct. 3, 1814. J. Murray.

P.S. One of the desiderata in tanning appears to be some good substitute for lime in the process of depilation. Would not this be effected by a stove heat?

Article VII.

On Galvanism as a Solvent of Urinary Calculi.
By Mr. William Stark.

(To Dr. Thomson.)

SIR,

A method of decomposing urinary calculi by the galvanic influence having been suggested by a Mr. Goring in the last number of your *Annals of Philosophy*, p. 361, I am induced to trouble you with some of the remarks which I made in a paper read at the Norwich Philosophical Society, Nov. 26, 1812, on that subject. I have no wish to take from Mr. Goring any merit that belongs to him in proposing a substitute for one of the most dangerous operations in surgery; but I must confess I do not see that Mr. Goring's suggestion promises much advantage, either with respect to the greater safety, or the alleviation of the sufferings, of patients. I would ask Mr. G. whether puncturing the bladder with a trochar, and introducing the appendages of a galvanic battery through the wound, is not attended with as much pain, and as much danger, as attend the common method of operating for the stone? Mr. G. considers, very properly, his mode of operating to be a work of time; and that consideration surely is sufficient to reject his plan; for how is it possible to keep a wound open a sufficient time to enable the action of the galvanic influence to decompose a calculus, without the greatest danger to the patient?

The plan I suggested to the Norwich Society was this:—"Let an instrument similar in shape to a common catheter, made with gum elastic, waxed silk, or any non-conducting substance, with two cavities, one for the wire from the positive end of the battery, the other for the wire from the negative end. This instrument with the wires to be introduced along the urethra into the bladder till it touches the stone, the wires having elasticity, and an inclination to bend outward, would facilitate the operation, as they would then be easier fixed, one on each side the calculus. When so fixed, which a surgeon accustomed to similar operations could easily determine, the patient might be left at rest, and the action of the battery suffered to proceed. It is not to be supposed that in all cases of calculous affection this plan is advisable, but in those where the bladder contains but one concretion: and from the best information I can procure, it seems that, on the average, six-sevenths of those afflicted have but one concretion, I have no doubt of its success. The patient immediately after, or towards the close, of the action of the battery, should be suffered to drink plentifully of diluents, that the disunited matter may be carried off by copious evacuations of urine."

When the above suggestions were made, I intended to have tried experiments to ascertain the time and intensity that certain calculi required for their decomposition, by first analyzing the different strata of them, and submitting them to the action of batteries of different powers; but the difficulty of procuring calculi, and the want of leisure, have at present frustrated my intention.

The objection which Mr. Goring makes to the introduction of any instrument up the urethra surely is not well founded, and certainly not liable to the risk, pain, and inconvenience, of the method he proposes. I dare say every person is acquainted with some one in the circle of his friends who is obliged to perform such an operation himself daily, at least it falls to my lot to know many who are obliged to do it, and without the least danger. However, so satisfied am I that the power of the galvanic influence is sufficient to accomplish the desired object, that were I so unfortunate as to be afflicted with the malady, I would cheerfully submit to a trial of it. Experiment must prove whether the plan which I suggested in 1812, Mr. Goring's, or any other, is best adapted for its accomplishment.

"Palmam qui meruit ferat."

Yours,
WILLIAM STARK.

Norwich, Nov. 12, 1814.

Article VIII.

Answer to Mr. R. Phillips's Animadversions on Mr. Hume.

(To Dr. Thomson.)

Sir,

Your Correspondent, Mr. R. Phillips, affords another example of human imperfection, that we are all "prone to complain" and often to publish too, without any reason or foundation.

Much of his valuable time, and more than nine-tenths of his letter to you, might have been spared, had he only looked into one of the volumes of your own System of Chemistry. There he would have found that, instead of dashing into a stream of words, studied expressions and polished sentences, you have in a single line said all that was necessary to correct where there was assuredly on my side no intentional plagiarism; for, speaking of the super-sulphate of barytes, you say in a note, "Mr. Hume has also mentioned it, but the fact was well known to chemists." To your decision I have long ago assented, and have also abandoned all claim to priority respecting the super-sulphate of strontian, after I found that Mr. Clayfield, of Bristol, had anticipated my observations.

Mr. R. Phillips is equally unfortunate in the two experiments quoted from M. Sage; he has drawn inferences diametrically opposite to those of all the chemists who have written upon the subject, especially those of France. In these experiments there is neither *water of solution* nor *water of crystallization*; there is no guide to direct future operators to avoid such errors as I shall presently quote; nor is there any room for asserting, that putting a quantity of *carbonate* of barytes into either of the two acids, although it be granted that in such a case it must be converted into a nitrate or muriate, is precisely the same as adding these salts or more particularly their solutions to the respective acids. There is nothing here to invalidate what I have said on the subject; nothing detailed by M. Sage to show the direct meaning and tendency of the instructions which I took the liberty to offer, and which may be thus epitomized:—*That such is the avidity of nitrous and muriatic acid for water, that they will attract even the whole of the water of solution from their respective barytic salts.*

When I published my paper upon this subject, it had been a common practice to ascertain the purity of nitrous acid, and even to purify it, by dropping into it a solution of nitrate of barytes; and this method was pursued by some very eminent professional men. I recollect one case in which a Gentleman, well known as a chemist and mineralogist, condemned some nitrous acid which had been sent to Sir John St. Aubyn, which, on my proving the error, was not returned. This happened above 14 years ago, and consequently was subsequent to M. Sage's experiments. Indeed, from the letter to you, I have no doubt but Mr. R. Phillips himself has often committed the same blunder.

That M. Bouillon-Lagrange did not benefit by M. Sage's example as he might have done by mine, will be evident to Mr. R. Phillips by the following sentence from Manuel d'un Cours de Chimie: " Ce sel (nitrate de baryte) est utile pour reconnaitre la presence de l'acide sulfurique. On peut s'en servir pour separer cet acide, qui se trouve quelquefois dans l'acide nitrique, et qui empêche de l'employer dans des experiences exactes."

I shall select another case, out of a great many I could name: it is that of Dr. Swediaur, who, as well as M. Bouillon-Lagrange, must have been even personally acquainted with M. Sage, and could not be ignorant of the story of carbonate of barytes in the acids, which may now be called Mr. R. Phillips's fable. In order to divest nitrous acid of sulphuric acid, the Doctor prescribes the following plan. Let a solution of nitrate of barytes be added until there be no more precipitation, " donec nihil amplius præcipitetur." I trust this experiment will be closely examined by Mr. R. Phillips, and that he will favour your readers with the result, the *nihil amplius*; and, for my own part, it is of no moment in this experiment whether the acid contain sulphuric acid or not, for I will venture to predict that the experiment will prove equally amusing to Mr. R. Phillips or to any other operator.

Discoveries and improvements in science invariably precede the dates of their being presented to the public, depending on the disposition, convenience, and pursuits, of their authors, and various other causes. In my own case these intervals have generally been of many years, and that particularly of employing *silver* as a test for *arsenic*, and asserting its superior efficacy, was not published until above 20 years after I had discovered it. It occurred to me while examining certain materials used in the preparation of carmine, and its utility was more distinctly evinced in subsequent trials, especially while analyzing a metallic ore belonging to the late Judge Buller. I might indeed appeal to many living witnesses that I am not so forward to tease the public with my writings as Mr. R. Phillips would insinuate. The Journals of the Royal Institution will bear testimony that I can prefer a *private* communication. I am unwilling to bring names forward on this occasion without permission of the parties; but I can recollect one case in point, and not unlike one of the experiments of M. Sage, which took place more than ten years before my remarks on barytes were published. Few philosophers have contributed more effectually to the Philosophical Transactions than the Gentleman to whom I now allude, and he probably has not forgotten the short conversation that passed in his library, at that time in the same street and very near to my residence, on our inspecting a vessel containing carbonate of barytes in powder mixed with nitrous acid, and in a state of apparent quiescence. I am persuaded that this Gentleman did not then consider me at all ignorant of any thing respecting M. Sage's experiment, or that I did not know in 1792 what I published in 1802 respecting barytes.

The two physicians to whom Mr. R. Phillips alludes cannot be more respected by him than by me; their reputations stand very high, and most deservedly so; they need no panegyric from either of us, and can fight their own battle; therefore any allusion to them on the present occasion is both irrelevant and intrusive. Such an interposition is more befitting an hireling than one in pursuit of the truth, and there are various ways of engaging such characters; for even flattery, ambition, malevolence or jealousy, is often as effectual a stimulus as any thing of a pecuniary nature. Who Mr. R. Phillips is, whether the initial be meant for *Robert*, *Richard*, *Ralph*, or *Roger*, I have yet to learn. I am totally unconscious of having offended any Gentleman of the name of Phillips, much less any one of whom I can boast a personal knowledge.*

I shall now expect to be told that I have also been forestalled respecting my test for arsenic; that the arseniate of silver, the *brick-red* coloured compound, had been prepared by others; that it had been formed before I was born, having been found in the laboratory of that busy old being, Dame Nature; that Henckel, Bergman, and others can bear testimony to the fact; and that M. Klaproth had frequently got hold of it, analyzed it, regenerated the same compound by means of nitrate of silver; but was so cruel and unlucky as to disregard the silver as a *test*, always preferring the *acetate of lead*, even to the end of the second volume of his valuable Analytical Researches; thus depriving Mr. R. Phillips of another theme for his peevish effusions.

Upon the whole, Sir, and from the last paragraph of the letter, I think your Correspondent writes more from principles of enmity and revenge than from a desire to improve science; and, as he seems to hold out a threat, I must be prepared to repel such attacks as I may now expect from one who is capable of treating me with so much malice and so little candour.

I remain, Sir, your obedient servant,

Long Acre, Dec. 12, 1814. Jos. Hume.

Article IX.

Notices respecting the Old Silver Mine in Linlithgowshire.

By John Fleming, D.D. F.R.S.E.

In the centre of the county of Linlithgow there is a small mountain group, the most elevated portion of which is known by the name of the Hilderstone Hills. Cairn-paple, or Cairn-naple, the highest of these hills, rises 980 feet above the level of the sea, and commands an extensive prospect of the surrounding country. The summit is flat, and is composed of green-stone, in many places passing into basalt and wacke.

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* Mr. Richard Phillips of the Poultry has been an acquaintance of mine these dozen years, and I have always considered him as one of the acutest chemists in London. He is well known to the public by various important papers in the Philosophical Magazine, and by his strictures on Dr. Powell's translation of the last edition of the London Pharmacopœia, one of the acutest pieces of criticism in the English language.—T.

The base on each side consists of rocks having the same dip and direction, and belonging to the Independent Coal Formation. At the western base there are many valuable beds of black coal and sand-stone, and at the eastern base there are extensive strata of lime-stone. These all stretch to the north-east, and have a westerly dip. The strata of lime-stone form a bed upwards of 30 feet in thickness, and are covered with beds of sand-stone, slate-clay, and clay-iron-stone. In these the vein is situated which is stated to have produced at one time a considerable quantity of lead and silver.

The lime-stone is of a blackish-grey colour, of various degrees of intensity. Its lustre is in general glimmering, often glistening, and even shining, but seldom dull. The compact fracture which it exhibits is in general fine splintery, often conchoidal, and sometimes earthy. It rarely occurs with a small granularly foliated fracture.* When the stone contains many petrifactions of entrochites, the foliated fracture is often conspicuous. It is opake, or very faintly translucent at the edges.

The lime-stone contains many irregular masses of flint, and the same mineral not unfrequently occurs in thin beds, thus occupying the same place in compact lime-stone which quartz is often observed to hold in granular lime-stone. The petrified remains of marine animals frequently present themselves in this rock. The teeth of fishes, particularly the *shark*, the spines and portions of the crust of *echini*, and fragments of the *trilobite* of Mr. Parkinson, are but rarely found. The remains of corals and shells are more abundant. The corals belong chiefly to the genera *fungia*, *millepora*, *eschara*, *orbitolites*, and *tubipora*. The shells are principally the remains of acephalous mollusca, some of which may be referred to the following established genera: *pinna*, *modiola*, *corbula*, *terebratula*, *gryphæa*, and *productus*. There are likewise a few shells belonging to genera in the cephalous order of mollusca. Thus there are species of the genera *turbo*, *melania*, *nautilus*, *ammonites*, and *orthocera*. In the month of May last I transmitted to the Wernerian Society a description of ten species of *orthoceratites* from the strata of this district, and chiefly from the bed of lime-stone abovementioned. This establishes the fact of their occurrence in the Independent Coal Formation, and thus proves that they are not peculiar to the lime-stones of the transition period. This point had been ascertained in Scotland upwards of 20 years ago by the late

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* One specimen of granularly foliated lime-stone from this bed is of a greyish black colour, and is much impregnated with bituminous matter, a small portion of which pervades the whole bed.

Rev. David Ure, Minister of Uphall, in Linlithgowshire. This Gentleman, in his History of Rutherglen and East Kilbride, published at Glasgow in 1793, describes and figures two species of this genus which he found in strata belonging to the great coal-field of Lanarkshire.

Having stated these facts in illustration of the natural history of the strata which the metalliferous vein is said to have traversed, I shall now mention a few circumstances concerning the vein itself with which I have become acquainted. Sir Robert Sibbald, in his Scotia Illustrata, published in 1684, part first, page 31, gives the date of its discovery, and records the name of the discoverer: "In Lothiana Occidentali, ad tria milliaria a Limnucho Austrum versus, in monte qui Cairne-papel dicitur, tempore Jacobi Sexti primi Britanniæ Monarchæ, ab Alexandro Mund Carbonario inventa est Argentifodina, ubi purius argentum, idque majore proportione, ex lapide rubro extractum fuit." The same author, in his History of the Sheriffdome of Linlithgow, 1710, p. 27, adds a little to his former description: "In *Hillderstone* Hills is the *silver mine*, which afforded much silver at the first working of it: a part of the melting-house is yet to be seen: and amongst the *adites* to the mine, the richest was that called God's Blessing. The spars are of different colours; some are white, and others of a red colour." Tradition says that this mine was abandoned in consequence of the roof of the workings falling down, and a great increase of water taking place. In hopes of overcoming these obstacles, the proprietor, the Earl of Hopetoun, some years ago made an attempt to re-open the mine. He brought some workmen from his mines at Lead Hills, and employed them in boring and sinking shafts in the neighbourhood of the old workings. But the information thus obtained was considered of little importance, and a stop was put to all further investigation.

The ruins of the old smelting-houses are still visible, and considerable heaps of rubbish surround the openings of the old shafts; but as no access to the mines can now be obtained, no precise information can be procured concerning the quantity or value of the ore, or the constitution and extent of the vein. In a lime-stone quarry about 200 yards to the east of the old workings the outgoing of a vein is distinctly seen, which traverses the strata in the direction of their dip, and is filled with soft clayey marl, and contains masses of impure lime-stone, together with lead glance and heavy spar. It runs towards the place where the former workings were carried on, but does not appear to have been a portion of the principal vein, as the old shafts have not been sunk in the direction of its line of bearing. Judging from such circumstances, the principal vein must have traversed the strata in the line of their stretch. To the south of the old workings there are indistinct appearances of a vein of green-stone running in a northerly direction; but the nature of this vein cannot be ascertained by inspection of the surface, which is deeply covered with soil. The heaps of rubbish in the neighbourhood of the mine are now our surest guides, and in them the following minerals may be observed :—

1. *Heavy Spar.*—This appears to have occurred in great quantity in the vein, and may even be observed filling up some small rents in the adjacent rocks. It is usually of a reddish colour, sometimes white, and presents the following sub-species. *Heavy spar earth,* found as a coating to the cavities of the other sub-species. *Granular heavy spar* is very abundant, and in some specimens appears to pass into compact heavy spar. *Curved lamellar heavy spar* is in small quantity at the old workings, but occurs abundantly in the cross vein mentioned as opening into the lime-stone quarry. *Straight lamellar heavy spar* appears in greatest plenty, usually compact, sometimes crystallized in the form of a rectangular four-sided table, having all the terminal planes levelled. In these sub-species of heavy spar the ores of the following metals occur either imbedded or disseminated.

2. *Lead.*—Lead glance was the ore sought after, and yielded so considerable a proportion of *silver* as to bear the expense of extraction. All the pieces of this ore which I have seen are broad foliated.

3. *Nickel.*—Both the ores of this metal are to be found here, but in small quantity. The *copper nickel* is in roundish pieces, from the size of a pea to that of a pigeon's egg. The *nickel ochre* sometimes occurs as a coating to the preceding species, and likewise fills small cells in the heavy spar, where it appears to hold the rank of an original deposition.

4. *Cobalt.*—I have not observed any of the alloys or oxides of this metal; but the arseniate of cobalt, or *cobalt crust,* fills the cavities of the heavy spar, and is spread as a coating on its surface.

5. *Zinc.*—The only ore of this metal which is here observable is a small portion of *brown blende.* The same ore associated with lead glance. Iron pyrites, brown spar, and lime-spar, may be observed in many of the small veins which traverse the strata of lime-stone in this district.

Amidst the rubbish may also be observed masses of calcareous sand-stone and indurated clay. After heavy rains, when fresh portions of the rubbish have been exposed, the poultry which feed near the place are observed to sicken and die. Are we to consider the cobalt crust as the cause of the mischief?

The rarest ores enumerated above are those of nickel and cobalt. These have been found in other places of Scotland, but not situated in the same kind of rocks. Some time ago I observed among the manuscripts of the late Dr. Walker, Professor of Natural History in the University of Edinburgh, a short notice of his having between the years 1761 and 1764 found copper nickel and nickel ochre in the mines at Lead Hills and Wanlockhead. These mines are situated in transition rocks. Hence we must consider nickel as belonging not only to the primitive class of rocks, but to the transition, and even to a new member of the floetz class.

At the old silver mine at Alva, in Clackmananshire, cobalt crust occurs in the cavities of heavy spar associated with native silver. The vein is situated in trap rocks, which are included in the *old red sand-stone*.* I have heard that the ores of cobalt occur in other parts of Scotland; but at present I am ignorant of the nature of the rocks in which they have been found.

I regret that these notices are so imperfect and unsatisfactory in those particulars which have a reference to the nature of the vein. But as they establish the existence of the ores of nickel and cobalt as inmates of the Independent Coal Formation, I have ventured to communicate them to your readers.

Manse of Flisk, Fifeshire,
Nov. 12, 1814.

Article X.

An Address to those Chemists who wish to examine the Laws of Chemical Proportions, and the Theory of Chemistry in general. By Jacob Berzelius, M. D. F.R.S. Professor of Chemistry in Stockholm.

Mr. Dalton has published in the *Annals of Philosophy*, vol. iii. p. 174, Observations concerning my memoir On the Cause of Chemical Proportions. It has given me pain to think that the respectable Dalton has taken my ideas on the corpuscular theory as a criticism on his, between which he has pointed out the difference. I think I have expressed myself in that memoir with sufficient precision to make the reader sensible that I neither meant to give the opinions of Dalton, nor a correction of them. There is a very essential difference between the researches of Mr. Dalton and myself. Mr. Dalton has chosen the method of an inventor, by setting out from a first principle, from which he endeavours to deduce the experimental results. For my own part, I have been obliged to take the road of an ordinary man, collecting together a number of experiments, from which I have endeavoured to draw conclusions more and more general. I have endeavoured to mount from experiment towards the first principle; while Mr. Dalton descends from that principle to experiment. It is certainly a great homage to the speculations of Dalton if we meet each other on the road.

Among the numerous experiments which I have myself made relative to this subject, there are some which do not appear to agree with the atomic theory so well as the rest; and which of course I have not been able to explain in a satisfactory manner. There are others whose existence is not a necessary consequence of the atomic theory of Mr. Dalton. These, in my opinion, prove that there is still something wanting in that theory, and which must be added to it in order to render it more complete. In my memoir On the Cause of Chemical Proportions I have endeavoured to draw the attention of the reader to these difficulties. Mr. Dalton has endeavoured to remove them, but with a levity which I did not expect from him, and which appears to me injurious to the science. He begins with pointing out the reasons why he cannot be of my opinion respecting the relative size and form of the atoms, &c. I must observe that at the bottom of every speculation in the exact sciences there always remains something which cannot be verified by experiment, and on which, of course, the imagination is at full liberty to indulge. The reveries of one man may be more ingenious, more interesting, and more probable, than those of another; but the science never gains any thing by disputes about subjects which are not susceptible of proof. I shall therefore pass over that part of the question in silence.

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* I have reason to conclude, from observations which I made this summer, that the country between the Lomonds on the south, and Stonehaven on the north, including the Ochils, and the hills in the neighbourhood of Perth, Dundee, Red-head, and Montrose, is composed of rocks belonging to the old red sand-stone. Upon the southern extremity of these rocks the great coal-field of the Forth rests, and occupies the situation of a newer deposition.

Mr. Dalton states that the electro-chemical polarity of the atoms makes no necessary part of the atomic theory, such as he maintains; nor did I ever mean to convey any such idea to the reader. For my own part, in considering a corpuscular theory of chemistry, I conceived that it should constitute the fundamental theory of the science; and instead of being occupied with a part of the phenomena, ought to embrace the whole. But when we treat of atoms in a chemical theory, we ought to endeavour to find out the cause of the affinity of these atoms. We ought to endeavour to combine researches respecting the cause why atoms combine with researches into the cause why they combine only in certain proportions. I do not consider the conjectures which I hazarded on the electro-chemical polarity of the atoms as of much importance. I scarcely consider them in any other light than as an ideal speculation deriving some little probability from what we know of the chemical effects of electricity. Yet the ideas on the relation of atoms to their electro-chemical properties, ought in my opinion to constitute an essential part of the corpuscular theory of chemistry, such as I view it; because I consider it as the duty of a man of science to endeavour to reach the first principle of the science, even though it should be actually impossible to attain it.

Mr. Dalton disapproves the idea which I announced, that we ought not to suppose an atom composed of 2 A + 2 B, 2 A + 3 B, &c. He thinks that such combinations take place, though but seldom. He inclines even to the idea that olefiant gas may be composed of two atoms of carbon to two of hydrogen, placed in the form of a rhombus, those of hydrogen being at the extremities of the longest diameter. Is there a chemical fact which countenances such an idea of the construction of the atoms of olefiant gas? If there be, the notion may be considered as an interesting conjecture; but if there be no such fact, the notion is a mere dream. There can be no doubt that if we give free liberty to our imagination in this manner, the science will degenerate into a mass of vain speculations, of no utility whatever, because founded on nothing but imagination. My idea, that in every inorganic combination one of the elements enters as unity, is founded on the circumstance that in all the inorganic bodies which I have analyzed, and I have analyzed a great number, I have found it to be so. Besides, if this were not the case, it is evident that all traces of chemical proportions would disappear in combinations which consist of various oxides, and could only be perceived in the most simple combinations, I have then founded my opinion on experience. Can Mr. Dalton produce an instance in which this opinion is obviously inaccurate?

Mr. Dalton then proceeds to remove the difficulties which I found attached to the corpuscular theory. I shall pass by the first, which he finds that I myself have obviated in a manner conformable to his opinion. The second difficulty consists in this—I have found compounds which are represented, for example, by $A\,O^3 + 1\frac{1}{2}\,B\,O$, which is contrary to the views of the atomic theory; while a combination of $A\,O^3 + B\,O^2$ does not exist, although it be conformable to that theory. Mr. Dalton removes the first part of these difficulties, by saying, " the body B in such a case has in reality three atoms of oxygen for one of metal, and the union in question is $2\,A\,O^3 + B\,O^3$." I have given two examples of which Mr. Dalton has said nothing. These examples are the subarseniate of lead ($As\,O^6 + 1\frac{1}{2}\,P\,O^2$), and the subsulphate of copper ($S\,O^3 + 1\frac{1}{2}\,Cu\,O^2$). It is evident that neither of these examples admits the explanation by which Mr. Dalton has endeavoured to remove the difficulty; for neither the oxide of lead nor copper can contain three atoms of oxygen. Mr. Dalton adds merely, that he considers with Proust, minium as a compound of yellow oxide and brown oxide of lead; from which it seems to follow that he considers the subarseniate of lead as $As\,O^6 + 3\,P\,O$. But what reason has Mr. Dalton to consider minium as composed in this manner? The necessity of it for his theory? But this necessity proves nothing as long as the theory itself is sub judice, which it probably will be for a long time to come. We may likewise explain the subsulphate of copper by supposing sulphuric acid composed of $S + 6\,O$, or black oxide of copper $Cu + O$, and of course the protoxide $2\,Cu + O$. But here lies the difficulty; for we have other reasons for considering the acid as $S + 3\,O$, and the oxide $Cu + 2\,O$. These difficulties cannot be removed by a stroke of the pen. Yet I am persuaded that both myself and Mr. Dalton will in time make use of these very difficulties to determine the true number of atoms in such and such compound bodies; but we must in the first place make a much greater number of experiments on these subjects than we possess at present; for it is not speculation alone, but experiment guided and accompanied by speculation, that can give us new information. Let me be allowed here to give an example. Hydrate of iron is so composed that the oxygen of the oxide is twice that of the water; but both Mr. Dalton and myself give to this oxide three atoms of oxygen. This hydrate, then, is $F\,O^3 + 1\frac{1}{2}\,H\,{}^2O$. I should certainly be disposed to explain this at once, by supposing that the oxide of iron contains six atoms of oxygen. But in order to see whether there be other proofs for such an idea, let us run over the combinations of the oxide of iron with other bodies. Let us examine, for example, the combinations of this oxide with acids which contain six atoms of oxygen, but which in their neutral saline combinations contain only three times as much oxygen as the base. If in these neutral combinations with these acids, the red oxide of iron does not constitute an exception to the general law, it ought likewise to contain six atoms of oxygen; for otherwise in the arseniate or chromate of iron there would be for each atom of iron half an atom of arsenic or chromium. Let us extend these researches still farther, and examine if the arseniates, chromates, &c. can be formed with oxides in which there is evidently no more than three atoms of oxygen, &c. The ultimate result of our researches will probably be that four and six atoms of oxygen, instead of three, are much more general than we have hitherto supposed; and that not only oxide of iron, but silica and various other oxides contain in fact six atoms of oxygen instead of three. I suppose that Mr. Dalton will agree with me that by such researches we may render much more complete the beautiful theory for which he feels himself so much interested, and for which we are in a great measure indebted to him.

I have considered it as a great difficulty attached to the atomic theory that it does not explain the laws concerning the combinations of the oxides with each other, namely, why the oxygen in the one is always a multiple by a whole number of that in the other. Mr. Dalton removes this difficulty by saying, "It is not the peculiar business of the atomic theory to explain it any more than to show why all the metallic oxides do not mutually combine with each other." But we must recollect that the principal circumstance in favour of the atomic theory is, that it gives a mechanical and very satisfactory cause why elementary atoms unite only in proportions which are multiples of each other. We have observed likewise that the compound atoms, that is to say, most of those which contain oxygen, combine likewise in a *multiplex ratio*, provided we attend only to the oxygen which they contain. It is clear that this must be owing to a cause similar to that which occasions similar proportions between elementary atoms; but the atomic theory throws no light on the subject. This must be admitted as something still wanting in the theory; for when a theory only explains one half of the phenomena which result from the principle which regulates these phenomena, it is surely incomplete. I differ, then, from Mr. Dalton, and must continue to consider the atomic theory as imperfect, and as clogged with difficulties, till it give us satisfactory explanations of all the phenomena relative to the chemical proportions. I do not think that we are very far from this explanation, but this is not the place to dilate upon the subject.

Mr. Dalton continues to observe, that the difficulty presented by the new oxide of iron of Gay-Lussac has been employed by me in a manner "particularly unfortunate." Had not Mr. Dalton thought it of importance to the science to express his opinion of the first pages of my memoir, before he was acquainted with its general tenour and termination, he might have seen how I myself removed the difficulty in question, both in the memoir itself, and by my analysis of the magnetic iron ores, where I have shown that the numbers given by Gay-Lussac are inaccurate. (Försäk til et System för Mineralogien. Stockholm, 1814. p. 97.*) As to the expression which Mr. Dalton has employed, "particularly unfortunate," I should have been glad to be deceived respecting it, by too little knowledge of the exact meaning of the phrase.

Mr. Dalton finishes his observations by endeavouring to remove the difficulties which the composition of organic bodies presents, in which the number of elementary atoms is often very great. I had instanced the atom of oxalic acid as an example of the composition of organic bodies. The method employed by Mr. Dalton to remove that difficulty deserves attention. "Were it," says he, "a matter of necessity, an anatomist might conceive one atom of hydrogen surrounded by nine of carbon, and the compound globule to have 18 of carbonic oxide adhering to it. But this would be an atom truly formidable in every sense of the word, as the least friction must be supposed capable of producing a violent explosion of such a mass of elasticity. I cannot, however, doubt that Dr. Berzelius having resumed the consideration, will very soon discover and acknowledge that his analysis is incorrect. In the mean time, I shall give my reasons for believing it to be so."

The tone of confidence with which Mr. Dalton finds me in the wrong in this passage has surprised me a little, and so much the more, that "any chemist is competent to satisfy himself on this head without appealing to authorities." As to the organic atoms, and the difficulties which they present, it would be too long to discuss them here. I therefore refer the reader to a set of experiments on the subject, which I am at present publishing in the *Annals of Philosophy*. As to my experiments on oxalic acid and oxalate of lead, I acknowledge that on repeating them I have found slight inaccuracies; but none of these have been in favour of Mr. Dalton's opinion. I have found that the oxalate of lead had given me rather too little oxide of lead, and oxalic acid rather too much hydrogen. Mr. Dalton has candidly stated his manner of analyzing the oxalate in question, and the result which he obtained. I shall likewise state mine. I prepared oxalate of lead by pouring a solution of pure oxalic acid into a solution of pure and neutral nitrate of lead. I did not employ acetate of lead, because that salt combines in part with all the substances which precipitate from it, as I have shown in my experiments on carbonate of lead, and more particularly in my Essay on Organic Combinations. Oxalate of lead contains no water of combination; hence it is easily dried, and requires only to be heated a few degrees above the temperature of the atmosphere. I burnt it in a capsule of thin glass, of which I had exactly determined the weight. I heated it in the flame of a spirit lamp, taking care to apply the first heat near the edge of the glass, and advancing gradually to the centre. The oxalate takes fire and burns quietly. When the whole is burnt I allow it to cool. I now weigh the glass with the oxide which it contains. This oxide is mixed with a small quantity of metallic lead, reduced by the charcoal of the acid. I dissolve it in distilled vinegar, wash the residual lead; dry it, and weigh it. To the weight of the oxide found, I add the quantity of oxygen requisite to convert the metallic lead into oxide. By this method of experimenting, oxalate of lead gave me 75·46 per cent. of oxide of lead. I would not recommend to the reader to repeat this experiment in metallic vessels, as of platinum or iron. When platinum vessels are used, I find that nine times out of ten the reduced lead unites with the platinum and spoils it. As to iron, every body knows that it increases in weight in the fire. As to the water which I found in effloresced oxalic acid, and to the difference between the result of the analyses of oxalate of lime, and of my analysis of oxalate of lead, Mr. Dalton will give me leave to refer him to the experiments of those, who in their analysis of oxalate of lime, have not neglected the water of combination contained in that salt.

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* An English translation of this interesting work has been published, to which the reader is referred. T.

When I endeavoured to draw the attention of chemists to the difficulties in the atomic theory, it was not my intention to refute that hypothesis. I wanted to lay open all the difficulties of that hypothesis, that nothing might escape our attention calculated to throw light on the subject. I wished the experiments to verify the theory; and I should have considered it as absurd, if I had taken the opposite road. I placed beside the corpuscular theory, a theory of volumes; because that theory is in some measure connected with facts which may be verified. To those who think that the theory of volumes may be fatal to the corpuscular theory, I would observe, that both are absolutely the same thing; but that the theory of volumes has this immediate advantage over the other, that it may be more easily verified. Let us suppose for a moment, that the theory of volumes were absolutely demonstrated. We would then ask, what is the difference between a solid and a gaseous body? The answer would restore to the corpuscular theory its rights. It would be demonstrated by that of volumes. The only difference between the two theories consists in the words *atom* and *volume*, that is to say, in the state of aggregation of the elements.

Chemical proportions begin to be more generally studied; but chemists are not agreed about the laws which regulate them. One party waits the opinion of those whom they consider as authorities; and these last appear to suspend their judgment, because the opinion requires to be verified by experiments, the number and difficulty of which is revolting to their minds. Let me be permitted to point out here, what in my opinion ought to be the principal object of their examination.

Chemical proportions depend upon two cardinal points: 1. The proportions in which the elementary atoms unite. 2. The proportions in which the compound atoms combine. The first of these is a necessary consequence of the atomic theory; and as the number of combinations which it includes is very limited, almost the whole of them have been examined, without finding a single exception to the law. Hence this point is very generally admitted. The case is very different with the second point. It includes an almost infinite number of compounds, varying in their elements as to the number of compound atoms which they contain. I have examined a great number of these substances, and as they are for the most part composed of oxides, I paid particular attention to the oxygen which they contain. The laws which I considered myself as entitled to establish from these experiments are well known.

The first cardinal point being established we must endeavour to establish or refute the second. If the accuracy of my experiments be admitted, I think their number is such that the second point also may be considered as verified. If this accuracy be not admitted, it is obviously necessary to repeat my experiments, and produce other analogous ones. My experiments have informed me how very difficult and even painful this kind of labour is; but it is absolutely necessary. I know of only two chemists who have hitherto occupied themselves with this examination, namely, Mr. Vogel, of Bayreuth; and Dr. Thomson, of London. The first undertook the analysis of a set of double salts, mostly containing water of crystallization; that is to say, composed of three or four oxides. The results which he obtained corresponded with the law. Dr. Thomson has in a theoretic dissertation gone over a great number of saline combinations. He inclines to admit the law.

This law is of much more importance than may be supposed at first. It deserves therefore the most careful and impartial examination. Were it not for this law, no combination composed of several oxides could be calculated, nor any analysis verified in a decisive manner for the theory. For it is clear, that if there be combinations expressible, for example, by $2\,A\,O^3 + 4\,B\,O^2 + 7\,C\,O + 5\,H\,O$; or if nature were to allow us to change the numbers in the formulas in any manner whatever, in such a case, all idea of determinate proportions would disappear, in proportion as these combinations became more complicated.

It is likewise by means of the law concerning the combination of oxides with each other, that we can hope to penetrate into the secrets of organic composition. When that law is demonstrated, the theory of chemical proportions may be considered as established.

It is absolutely necessary, that every person who repeats an analysis in order to ascertain its accuracy, should be acquainted with all the requisites to make an exact analysis, in the same degree as the author of the work which he proposes to examine. Otherwise, he takes upon him to judge his master. Accurate chemical analysis is half an art, half a science. He who does not unite both in one, will never be in a condition to make an exact analysis. We must always chuse that method in which the precision of result depends the least on the dexterity of the operator. We must likewise be able, from long experience, to avoid all the circumstances by which the result would be rendered inaccurate, that neither from forgetfulness, carelessness, or ignorance, we may neglect to observe and remove them. These circumstances are generally of such a nature that chemists usually pay no attention to them; yet they are of considerable importance when our object is to obtain the utmost possible accuracy. The most skilful chemist will continue long an apprentice in the art of analysis, if he has not been in the habit, from the commencement of his studies, to attend to these circumstances. Long experience is even often necessary before we discover their existence, unless we have the advantage of being informed by others.

When we have to judge between two different analytical methods, or between two different chemists, we must be well acquainted with the methods employed; and both the method and the reputation of the author ought to enter for something in our judgment. If, for example, we have to compare two experiments on the composition of a metallic oxide, one of which was made by dissolving the metal in nitric acid in a phial, from which the acid was then evaporated, and the residue exposed to a strong heat: if the other was made by precipitating the solution in nitric acid by means of an alkali, filtrating, &c., it is presumable, that supposing the same dexterity in the operators, the first experiment is more accurate than the second, because it has been less exposed to the influence of circumstances. It is necessary, however, that he who judges of two results, should know the circumstances that may render the one or the other inexact. We must know, for example, that the first experiment can scarcely have any other error, than giving too great a quantity of oxygen to the oxide. This may be either owing to the application of too little heat, or (which is more usual) to the glass not having resisted the action of the nitric acid during the evaporation, so that a little nitre is formed, the acid of which increases the weight of the oxygen that we think we have found. (This circumstance long deceived me in my first experiments before I perceived it.) The other method of operating can have no other error than indicating too little oxygen, if ammonia has been employed to precipitate the oxide. But if a fixed alkali has been employed, the result may be too great, as well as too small; for if the precipitate has not been well washed, there will remain in it nitrate of potash or of soda. Or if too much alkali has been employed in precipitating, a portion of that alkali for the most part combines with the oxide, and cannot afterwards be removed by washing. But I should never have done, were I to point out here all the circumstances requisite to make an accurate analysis, and to judge of its accuracy.

It is obvious, that the theory of chemical proportions is intimately connected with the general theory of chemistry; but it is necessary, that those who labour either to examine it, to verify it, to extend, or apply it, should have their eyes constantly fixed upon the whole of chemistry. They must not adopt any theoretical explanation, till they have seen that it is not in contradiction with any other part of the theory, which we have reason to consider as well founded. This is a circumstance which chemists often neglect, though it is of the highest importance; and without attending to it, we cannot expect to make any valuable improvement in the theory of chemistry. Long custom is necessary, and a very extensive acquaintance indeed with chemistry, to have, so to speak, the whole science before our eyes, in order to be able to judge, without long reflection, whether an ingenious explanation of some phenomenon be inconsistent or not with some other part of the theory. It is from not attending to this circumstance, that chemists of the greatest distinction sometimes adopt ideas respecting some points, which are inconsistent with other opinions equally adopted by them.

I shall here give an example of this. Dr. Thomson, whose merits as a skilful chemist do not stand in need of any panegyric from me, has examined, (*Annals of Philosophy*, iii. 139,) the laws relating to the combination of oxides with each other; an examination for which chemists are much obliged to him. He then says, "the more I have examined this law, the more correct in general does it appear." In the same volume, page 106, speaking of *iodine*, this philosopher expresses himself as follows: "How much these new discoveries must alter the presently received chemical theory, and how they serve to confirm Davy's opinion respecting muriatic acid, is too obvious to escape attention." But had the author at this moment called to his mind the constitution of the submuriates with water of combination, he would have found that the laws concerning the combination of oxides, and the theory of Davy respecting muriatic acid, are absolutely irreconcileable; and that either the one or the other of these must be abandoned as erroneous. If, at this time, the external resemblance of crystallized iodine to the oxide of manganese crystallized had recalled the last of these to his mind, he would perhaps have found, that it is not more difficult to conceive how the first disengages an excess of oxygen to combine with the bases, than how the last disengages an excess of oxygen to combine with the acids.*

But I have allowed myself perhaps to be carried too far by ideas which have spontaneously followed each other in my mind. I hope, however, that I have drawn the attention of the reader to circumstances which may be of some importance in the examination of chemical proportions, as well as in the theory of chemistry in general.

Article XI.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North. Longitude West in Time 6″$\frac{82}{100}$.

1814.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Dec. 18	8h	50′	24°	17′	27″	1h	25′	24°	21′	31″	Not observed.	Not observed.
Ditto 19	8	45	24	23	37	1	45	24	24	36		
Ditto 20	8	30	24	21	50	1	30	24	21	52		
Ditto 21	8	45	24	19	17	1	45	24	21	03		
Ditto 22	8	45	24	17	28	—	—	—	—	—		
Ditto 23	8	45	24	16	29	1	20	24	21	33		
Ditto 24	8	45	24	15	26	—	—	—	—	—		
Ditto 25	8	45	24	17	00	1	20	24	22	07		
Ditto 26	—	—	—	—	—	1	20	24	20	48		
Ditto 27	8	45	24	16	28	1	25	24	20	05		
Ditto 28	—	—	—	—	—	1	25	24	19	55		
Ditto 29	8	45	24	17	13	1	35	24	19	35		
Ditto 30	—	—	—	—	—	1	35	24	20	28		
Ditto 31	—	—	—	—	—	1	30	24	19	45		

1814.

Mean of Observations in Dec.	Morning	at	8h	44′.....	Variation	24°	18′	02″	West.
	Noon	at	1	30.....	Ditto	24	20	36	
	Evening	at	—	—.....	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	41.....	Ditto	24	16	20	West.
	Noon	at	1	40.....	Ditto	24	20	37	
	Evening	at	—	—.....	Ditto	—	—	—	Not obs.

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* I must make a remark here. It has already struck me more than once, that Dr. Berzelius and myself reason from different principles; for he has frequently pointed out facts which he said were inconsistent with the theory of muriatic acid by Davy, when I myself could not perceive any inconsistency in them. Even in the present example, I feel myself in the same predicament. I am unable to see any inconsistency between the facts established respecting the submuriates, and the opinions I entertain respecting chlorine. But if Dr. Berzelius will state this inconsistency in plain terms, so that I can see what he means, I shall examine it, and if I find it to be as he says, I shall be very ready to embrace his opinion.—T.

Mean of Observations in Oct.	Morning	at	8ʰ	39′	..... Variation	24°	14′	08″	West.
	Noon	at	1	42	..... Ditto	24	21	45	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	32	..... Ditto	24	14	33	West.
	Noon	at	1	39	..... Ditto	24	23	17	
	Evening	at	6	19	..... Ditto	24	16	50	
Ditto in Aug.	Morning	at	8	30	..... Ditto	24	14	13	West.
	Noon	at	1	39	..... Ditto	24	23	48	
	Evening	at	6	57	..... Ditto	24	16	31	
Ditto in July.	Morning	at	8	41	..... Ditto	24	13	29	West.
	Noon	at	1	42	..... Ditto	24	23	44	
	Evening	at	6	58	..... Ditto	24	17	00	
Ditto in June.	Morning	at	8	44	..... Ditto	24	13	10	West.
	Noon	at	1	30	..... Ditto	24	22	48	
	Evening	at	6	52	..... Ditto	24	16	29	
Ditto in May.	Morning	at	8	45	..... Ditto	24	13	12	West.
	Noon	at	1	44	..... Ditto	24	22	13	
	Evening	at	6	38	..... Ditto	24	16	14	
Ditto in April.	Morning	at	8	45	..... Ditto	24	12	53	West.
	Noon	at	1	48	..... Ditto	24	23	53	
	Evening	at	6	29	..... Ditto	24	15	30	
Ditto in March.	Morning	at	8	52	..... Ditto	24	14	29	West.
	Noon	at	1	52	..... Ditto	24	23	08	
	Evening	at	6	11	..... Ditto	24	15	33	
Ditto in Feb.	Morning	at	8	47	..... Ditto	24	14	50	West.
	Noon	at	1	52	..... Ditto	24	20	58	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	52	..... Ditto	24	15	05	West.
	Noon	at	1	53	..... Ditto	24	19	03	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at	8	53	..... Ditto	24	17	39	West.
	Noon	at	1	51	..... Ditto	24	20	30	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	40	..... Ditto	24	17	17	West.
	Noon	at	1	54	..... Ditto	24	20	24	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	..... Ditto	24	15	41	West.
	Noon	at	1	59	..... Ditto	24	22	53	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	..... Ditto	24	15	46	West.
	Noon	at	2	02	..... Ditto	24	22	32	
	Evening	at	6	03	..... Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	..... Ditto	24	15	55	West.
	Noon	at	2	02	..... Ditto	24	23	32	
	Evening	at	7	05	..... Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	..... Ditto	24	14	32	West.
	Noon	at	1	50	..... Ditto	24	23	04	
	Evening	at	7	08	..... Ditto	24	16	43	
Ditto in June.	Morning	at	8	30	..... Ditto	24	12	55	West.
	Noon	at	1	33	..... Ditto	24	22	17	
	Evening	at	7	04	..... Ditto	24	16	04	
Ditto in May.	Morning	at	8	32	..... Ditto	24	12	02	West.
	Noon	at	1	37	..... Ditto	24	20	54	
	Evening	at	6	40	..... Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	..... Ditto	24	09	18	West.
	Noon	at	0	59	..... Ditto	24	21	12	
	Evening	at	5	46	..... Ditto	24	15	25	

Magnetical Observations continued.

1815.

Month.		Morning Observ.					Noon Observ.					Evening Observ.	
		Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Jan.	1	8h	50′	24°	17′	01″	1h	35′	24°	20′	04″	Not observed.	Not observed.
Ditto	2	8	50	24	17	50	—	—	—	—	—		
Ditto	3	8	50	24	17	24	1	35	24	19	46		
Ditto	4	8	50	24	19	20	1	55	24	20	32		
Ditto	5	8	50	24	18	00	1	35	24	20	50		
Ditto	6	8	45	24	17	02	—	—	—	—	—		
Ditto	7	8	40	24	15	48	1	40	24	19	19		
Ditto	8	8	45	24	16	53	1	40	24	20	43		
Ditto	9	8	40	24	16	50	1	35	24	19	23		
Ditto	10	8	40	24	16	35	1	45	24	17	46		
Ditto	11	8	45	24	14	22	1	35	24	19	00		
Ditto	12	8	45	24	15	58	—	—	—	—	—		
Ditto	13	8	50	24	17	04	1	35	24	19	30		
Ditto	14	8	45	24	18	08	1	20	24	20	37		
Ditto	15	8	45	24	16	30	1	25	24	18	26		
Ditto	16	8	40	24	17	02	1	35	24	19	41		
Ditto	17	8	45	24	16	42	1	50	24	20	01		

Comparison of the Variations in the Years 1813 *and* 1814.

		1813.			1814.			Difference.		
April	Morning	24°	09′	18″	24°	12′	53″	+	3′	35″
	Noon	24	21	20	24	23	58	+	2	41
	Evening	24	15	25	24	15	30	+	0	05
May	Morning	24	12	02	24	12	49	+	0	47
	Noon	24	20	54	24	22	13	+	1	19
	Evening	24	13	47	24	16	14	+	2	27
June	Morning	24	12	35	24	13	10	+	0	35
	Noon	24	22	17	24	22	48	+	0	31
	Evening	24	16	04	24	16	18	+	0	44
July	Morning	24	14	32	24	13	29	−	1	03
	Noon	24	23	04	24	23	44	+	0	40
	Evening	24	16	43	24	17	00	+	0	17
Aug.	Morning	24	15	55	24	14	13	−	1	42
	Noon	24	23	32	24	23	48	+	0	16
	Evening	24	16	08	24	16	31	+	0	23
Sept.	Morning	24	15	46	24	14	33	−	1	16
	Noon	24	22	32	24	23	17	+	0	45
	Evening	24	16	04	24	16	50	+	0	46
Oct.	Morning	24	15	41	24	14	08	−	1	33
	Noon	24	22	53	24	21	45	−	1	08
	Evening	—	—	—	—	—	—		—	—
Nov.	Morning	24	17	17	24	16	20	−	0	57
	Noon	24	20	24	24	20	37	+	0	13
	Evening	—	—	—	—	—	—		—	—
Dec.	Morning	24	17	39	24	18	02	+	0	23
	Noon	24	20	30	24	20	36	+	0	06
	Evening	—	—	—	—	—	—		—	—

Dec. 19.—In taking the mean of the observations for the month of December, the variation on the 19th is rejected, on account of its being unusually great.

Jan. 10, 1815.—The needles vibrated between three and four minutes; and in the night the wind blew very hard, with rain, from the west.

Jan. 11.—At the morning observation the needles vibrated three and four minutes. The wind still blew very hard, and continued during the day. At noon the vibration of the needles was seven minutes.

Rain fallen { Between noon of the 1st Dec. / Between noon of the 1st Jan. } 2·500 inches.

Article XII.

Analyses of Books.

Memoirs of the Wernerian Natural History Society, Vol. II. Part I. For the Years 1811, 1812, 1813. Edinburgh, 1814.

The Wernerian Natural History Society was founded in Edinburgh, in 1808, for the purpose of cultivating all the different branches of natural history; though, from the name given to it, we are led to suppose that geognosy occupied the chief place, at least in the opinion of the original founders; as that name is derived from Werner, the celebrated founder of that important science. We had no opportunity of noticing the first volume of Memoirs published by this Society, as it made its appearance before the commencement of the *Annals of Philosophy*. The present volume contains 20 papers, which we shall notice in their order.

1. *Outlines of the Mineralogy of the Ochil Hills.* By Charles Mackenzie, Esq.—This paper having been already published in the *Annals of Philosophy*, vol. iii. p. 116, we shall satisfy ourselves with a very short account of it here. The Ochils are a beautiful chain of hills, lying chiefly in Perthshire in Scotland, and dividing Strathern from the river district of the Forth. These mountains are all clothed with verdure, and many of them cultivated to the very top; hence the rocks of which they consist are seldom exposed, and the task of ascertaining their structure is attended with almost insurmountable difficulties. It is not surprising, therefore, that Mr. Mackenzie was unable to determine exactly the relative position of the different rocks, and consequently the class of formations to which they belong. For my own part, I consider the Ochils as very like the Pentlands in their structure, and think that both consist of a series of rocks, not exactly similar to any that Werner has described as constituting either the floetz or the floetz trap; though, the lowest beds excepted, they seem more connected with the latter than the former. I suspect that a

careful study of these two chains of mountains will occasion some alterations in the present Wernerian arrangement of flœtz trap rocks. The rocks of which the Ochils are composed are the following:—

1. Red sand-stone.
2. Amygdaloid.—Basis clay-stone.
3. Grey sand-stone.
4. Lime-stone.—Very uncommon.
5. Slate-clay.—Also in small quantity.
6. Clay-stone.—Abundant.
7. Tuff.
8. Basaltic clink-stone.—Peculiar to the Ochils.
9. Green-stone.
10. Clay-stone porphyry.—Common.
11. Felspar porphyry.
12. Compact felspar.—Common.

The veins which occur in these mountains contain calcareous spar, steatite, sulphate of barytes, iron, cobalt, silver, copper, lead.

Were those geologists, who consider all green-stone, basalt, and porphyry slate, as lava emitted from volcanoes, to examine the structure and relations of the Pentlands and Ochils, it would probably induce them to modify their opinions.

2. *A Geological Account of the Southern District of Stirlingshire, commonly called the Campsie Hills; with a few Remarks relative to the two prevailing Theories as to Geology, and some Examples given illustrative of these Remarks.* By Lieutenant-Colonel Imrie, F.R.S. Edin. — This district constitutes a part of the very extensive independent coal formation, which occupies so great a portion of the southern part of Scotland, extending from the German Sea at St. Andrews to the western parts of Airshire. The district described by Col. Imrie consists of ranges of hills running north and south, and separated by narrow valleys. He found that these hills are composed of beds which have the following position, beginning with the lowest bed.

1. Shale.
2. Dark bluish grey lime-stone with entrochi.
3. Slate-clay.
4. Sand-stone.
5. Slate-clay mixed with glance coal.
6. Dark bluish grey lime-stone.
7. Slate-clay.
8. Sand-stone.
9. Slate-clay.
10. Sand-stone.
11. A very thick bed of green-stone.

All these beds evidently belong to the independent coal formation, except the uppermost bed of green-stone, which from its position, and its being confined to the top of the hills, probably belongs to the newest floetz trap.

Col. Imrie accounts for this disappearance of the green-stone, by currents of water which formerly flowed over this district from east to west; and he points out traces of these currents, in scratches still remaining on the surface of the green-stone, and in the particular configuration of the hills. This is the ingenious speculation of Sir James Hall, to which he has given plausibility by his usual industry and address. But it is a very hazardous speculation, the sources of mistake being almost innumerable, and the means of correcting error very scanty. The speculation seems to have originated with Saussure and Dolomieu, who, however, were never able to make much of it.

Col. Imrie has entered into some discussions respecting the igneous or aqueous origin of green-stone. He has shown, by an example from the neighbourhood of Carthage, that columns may be formed in substances deposited from water; and by an example from the Lipari Islands, that they may be formed likewise from lava. Obsidian and pumice he considers as of volcanic origin. I should like much to know, whether the composition of the obsidian found in Hungary be the same with that found in Iceland, Lipari, and the Peak of Teneriffe. It is very likely that they would be found to differ; and that the dispute about the aqueous or igneous origin of this mineral, has originated from giving the same name to two different species.

3. *Chemical Analysis of a Specimen of Magnetic Iron Ore from Greenland.* By Thomas Thomson, M. D. F.R.S. L. and E.—The analysis of the ores of iron constitutes one of the most difficult parts of mineralogical chemistry. The present example, compared with the Swedish ores lately analyzed by Berzelius, is sufficient to show, that more than one species of magnetic iron ore exists. Berzelius's ores were combinations of black and red oxides of iron. My ore contained titanium, and from the quantity of red oxide of iron obtained, (126·5 from 100 of the ore,) it is obvious that part of the iron must have been in the metallic state. This is further confirmed by the effervescence of the ore with muriatic acid. The speculation, that the ore consists of an unknown protoxide of iron, is supported by too slight analogies to be admitted.

4. *Description of a Sword Fish found in the Frith of Forth, in June,* 1811. By William Elford Leach, Esq. F.L.S. W.S. &c. —To this species, Dr. Leach gives the name of Xiphias Rondeletii, because he thinks it was first discovered and described by Rondeletius. The Xiphias Gladius is likewise probably found in the Frith.

5. *Some Observations on the Genus Squalus of Linnæus, with Descriptions and outline Figures of two British Species.* By William Elford Leach, Esq. F. L. S. W.S. &c.—Dr. Leach divides the family of the squalinidæ into four genera, as follows :—

* *Pinna anali.*

1. Pinna caudalis lunata ....Squalus.
2. Pinna caudalis irregularis.........Galeus.

* *Pinna anali nulla.*

3. Os ante oculos situm ....Squatina.
4. Os pone oculos situm ....Acanthias.

The two species which he describes, are, 1. The *galeus mustelus*, the *squalus mustelus* of Linnæus. The specimen examined was caught in the Frith of Forth. 2. The *squalus selanonus*, from a dried specimen in the Museum of the University of Edinburgh, caught in Lochfyne, and described by the late Dr. Walker.

6. *An Essay on Sponges, with Descriptions of all the Species that have been discovered on the Coast of Great Britain.* By George Montagu, Esq. F. L. S. and M. W. S.—This is a very curious and elaborate paper; but from its nature, scarcely susceptible of useful abridgment. Mr. Montagu considers sponges as animals destitute of all motion, and possessed of organs of digestion similar to those of plants; in short, they are plants as to structure and properties, but they are of an animal nature; because, when distilled, they yield the same products with other animal bodies. He gives a description of no fewer than 39 species of British sponges. Only 14 species were previously known, so that he has nearly tripled the list. I wish he had been at the trouble to make his title grammatical. He has made it Spongia Britannica, instead of Spongiæ Britannicæ, which is the true title.

7. *Mineralogical Description of Tinto.* By Dr. Macknight.—Tinto is a mountain in Lanarkshire, about 2300 feet above the level of the sea. According to Dr. Macknight, it is composed of floetz rocks, which, probably, rest upon grey-wacke, of which the neighbouring hills are composed. The lowest bed is a conglomerate, having a basis of clay, with a dark greyish colour, and somewhat resembling an intimately mixed green-stone. The fragments it contains are water-worn masses of transition rocks, as grey-wacke, grey-wacke-slate, iron-clay, and common flinty-slate, with veins of quartz. There are also nodules of quartz, mica, felspar, splintery horn-stone and felspar, passing into conchoidal horn-stone. Over this conglomerate, which constitutes the base of the mountain, masses of clay-stone, green-stone, and green-stone passing into clink-stone and porphyry-slate, successively appear, till we arrive at the summit of the mountain, which consists of compact felspar, and felspar porphyry. These different rocks probably constitute so many beds, but the point was not ascertained.

8. *Short Account of the Rocks which occur in the Neighbourhood of Dundee.* By the Rev. John Fleming, Flisk.—These rocks are all floetz. A porphyry with base of compact felspar alternates with sand-stone, west from Dundee; and the sand-stone fills up several hollows in the porphyry. Further east, rocks of green-stone and clay-stone appear, and the green-stone seems to pass into clink-stone.

9. *Observations on the Mineralogy of the Neighbourhood of St. Andrew's, in Fife.* By the Rev. John Fleming, Flisk.—That part of Fife, which is on the north side of the Eden, consists of floetz formations; but the southern part of the county is composed of the independent coal formation. St. Andrew's lies at the north-east corner of the independent coal formation. The rocks there are of two kinds: 1. Rocks belonging to the independent coal formation. 2. Rocks belonging to the newest floetz trap. The first are sandstone, coal, slate-clay, and clay-iron-stone. Of the second, only two rocks are described, namely, basalt and trap tuff.

10, 11. *Meteorological Observations on a Greenland Voyage, in the Ship Resolution, of Whitby, in* 1811 *and* 1812. By William Scoresby, Jun. M.W.S.—These observations are very curious and valuable, though not susceptible of abridgment. The ship went nearly as far north as latitude 79°, or within 11° of the pole. The thermometer even in June was seldom much above the freezing point, and sometimes below it. In these climates no summer seems to exist.

12. *Analysis of Pearl Spar.* By William Hisinger, Esq., of Stockholm. This paper is printed in French, which I think rather injurious to the Memoirs of the Wernerian Society. It would have been better to have translated it. The result of the analysis is as follows:—

Lime	27·97
Magnesia	21·14
Oxide of iron	3·40
Oxide of manganese	1·50
Carbonic acid	44·60
Loss	1·39
	100·00

13. *Outline of the Mineralogy of the Pentland Hills.* By Professor Jameson.—The Pentlands are a well known and beautiful chain of mountains, situated immediately on the south side of Edinburgh. They stretch from north-east to south west, passing through the counties of Mid-Lothian and Peebles. The chain is upwards of 20 miles in length; but the portion described in this paper is confined to the north-east part of the chain, and constitutes a length of about eight miles. The rocks of which this portion are composed are of two kinds, namely, transition and floetz. The transition rocks lie lowest, and seem to constitute the basis of the Pentlands. Three transition rocks occur, namely, clay-slate, grey-wacke, and green-stone. The two first alternate. Mr. Jameson conceives that the grey-wacke is an original deposite, that it never was in the state of loose sand, afterwards cemented together by some unknown process; but was originally deposited from a liquid in the state in which we find it. This opinion, I conceive, is very likely to be correct. The rock itself bears scarcely any marks of being composed of fragments that had been subjected to

trituration in water; but I do not think that Mr. Jameson has been quite so fortunate in his name as in the idea itself. All rocks, which are still in the original state in which they were deposited, he calls *chemical deposites*; while he distinguishes those which have been in the state of sand, or fragments, and afterwards cemented together, by the name of *mechanical deposites.* Now a chemical combination means substances united together by chemical affinity; and one of the characteristics of this kind of combination is, that the constituents enter always in the same proportion. I am afraid, that according to this definition, very few rocks would be entitled to the name of chemical deposites, while some of those that Mr. Jameson would distinguish by the name of mechanical, (as shell lime-stone,) would be in reality entitled to the name of chemical. Some other phrase, (*immediate deposites,* for example,) might easily be chosen to denote the idea, which then, in my opinion, would be unexceptionable.

The floetz rocks, which constitute by far the greatest portion of the Pentlands, are conglomerate, sand-stone, clink-stone, clink-stone porphyry, amygdaloidal clink-stone porphyry, green-stone, compact felspar, clay-stone, clay-stone tuff, and porphyry.

The conglomerate rock is composed of portions of quartz, grey-wacke, grey-wacke slate, porphyry, felspar, flinty-slate, common jasper, horn-stone, and mica. Two varieties of it occur, a hard and a soft. It lies low in the hills, and Mr. Jameson supposes that it alternates with grey-wacke. The sand-stone covers, and probably alternates with the conglomerate. Mr. Jameson considers it as an immediate deposite. The clink-stone seems to be over and alternate with the sand-stone. It often contains crystals of felspar, and is then called clink-stone porphyry, or porphyry-slate. It passes into compact felspar. The green-stone is an uncommon rock in the Pentlands; but the compact felspar occurs in great abundance. It passes into clay-stone, which is a very copious rock, and often constitutes the summit of the different mountains. The porphyry, which is merely clay-stone containing crystals of felspar, often covers it.

14. *On conglomerated or brecciated Rocks.* By Professor Jameson. In this paper, Mr. Jameson endeavours to show, that many rocks hitherto supposed to be composed of fragments of older rocks cemented together, are in fact immediate, or, as he calls them, *chemical,* deposites from a liquid. The idea certainly deserves to be carefully examined. I have little doubt that, to a certain extent at least, his notions will be found correct; but it is difficult to reason on such a subject in a satisfactory manner, because the evidence can only be derived from an ocular inspection of the rocks in question. Conglomerated rocks occur in primitive, transition, and floetz formations. To the first belong conglomerated gneiss, conglomerated mica-slate, conglomerated granite, and conglomerated lime-stone. All of these Mr. Jameson considers as immediate deposites; because the supposed fragments pass insensibly

into the basis of the rock. The transition conglomerated rocks are grey-wacke, sand-stone, and lime-stone. He considers all these as immediate deposites, for a similar reason with the primitive conglomerates. The floetz conglomerates are sand-stone conglomerate, sand-stone, and trap tuff. The first and last he considers as always immediate deposites; the sand-stone as frequently so.

15. *On Porphyry.* By Professor Jameson.—Werner admits into his system of geognosy five kinds of porphyry; namely, clay-porphyry, horn-stone-porphyry, pitch-stone-porphyry, obsidian-porphyry, and pearl-stone-porphyry; all of which are primitive rocks. The object of this paper is to show, that there exist likewise transition and floetz porphyries. Transition porphyry has a basis of clay-stone or felspar. It contains crystals of felspar, grains of quartz, scales of mica, and crystals of hornblende. It occurs abundantly in Dumfriesshire, and between New Galloway and Dumfries, where it alternates with grey-wacke, grey-wacke-slate, and transition green-stone. *Floetz porphyry* has likewise clay-stone or felspar for its basis, and contains crystals of common felspar, glassy felspar, and quartz. It occurs in the islands of Arran, Roasay, and Skye, and in the Ochils and Pentland hills.

16. *Mineralogical Observations and Speculations.* By Professor Jameson.—These observations and speculations relate to three particulars: 1. Stratification. This first speculation is rather of a bold nature, as Mr. Jameson endeavours to explain in what manner the crust of the earth was originally formed. His notion seems to be, that the whole earth is a crystal, that the tabular masses or strata are the folia of this crystal. He even affirms, that these strata meet under determinate angles, and are to be considered as the same with the *cleavage* of common crystals. I am sorry that in this very uncommon and novel view of stratification, I cannot agree with the opinion entertained by my very ingenious friend. The laws of crystallization have been studied with sufficient care and success to enable us to decide, without hesitation, that the earth cannot be a crystal, and that its strata are not the same with the folia of crystals. The figure of crystals is owing to the figure of their ultimate particles, and the particular way in which they unite. Without chemical affinity and chemical combination we can have no crystal. The different plates of a crystal are arranged by the same law, and they are attached to each other by chemical affinity. But what chemical affinity can unite the different strata of the earth with each other? Where is the chemical affinity between lime-stone, slate-clay, coal, and sand-stone, for example; or between sand-stone, green-stone, slate-clay, or basalt? Yet what is more common that such alternations. To say that the strata form every where regular angles with each other, as happens with the plates of a crystal, is saying what has never been proved or even rendered probable. The last part of the speculation, that different strata are often of simultaneous formation, is, to a certain extent at least, more probable than the former part; though I think

that a more careful and rigid examination, than Mr. Jameson has bestowed on it, would be necessary in order to establish it, or even to render it probable. 2. On veins. Mr. Jameson considers veins as of two kinds. Some have been open rents afterwards filled up. Some have never been rents, but have been formed at the time the strata were deposited, or soon after, by a kind of crystallization in the rock. These he calls cotemporaneous veins, and he conceives them to be much more numerous than has hitherto been supposed. This opinion, which is very ingenious, is ably supported, and is, I think, very likely to be true. 3. On coal. Coal has been hitherto usually considered as vegetable matter, more or less changed in its nature; but Mr. Jameson believes that glance coal, and black coal, are original chemical deposites, as little connected with vegetable remains as the shells in lime-stone are with the lime-stone. His reasons are, that they occur in primitive rocks, as gneis, mica-slate, clay-slate, and appear to be of cotemporaneous formation with these rocks. Brown coal, on the contrary, he considers as of vegetable origin.

17. *Observations on the Natural History of the Colymbus Immer.* By Dr. Arthur Edmondstone.—This is a large bird which frequents the Zetland Islands in winter, but disappears in summer. It lives on the water, and seldom lands. It is the opinion of the Zetlanders that it cannot fly, and that it hatches its eggs under its wings. Dr. Edmonstone had an opportunity of refuting the first of these notions; for one, which he wounded, after diving, took to wing and flew a hundred yards, when exhausted by loss of blood it fell and was taken. It no doubt migrates to Greenland, or the North Cape, during the breeding season.

18. *Contributions to the British Fauna.* By the Rev. John Fleming, F.R.S.E.—Dr. Fleming gives a description of the following animals hitherto unknown, or nearly so, in Great Britain. 1. Sorex fodiens, or water shrew. 2. Pleuronectes punctatus. 3. Lepas fascicularis. 4. Hirudo verrucosa. 5. Echinus miliaris. 6. Lucernaria fascicularis. 7. Caryophyllia cyathus. 8. Fungia turbinata. 9. Flustra ellisii. Curious information is given respecting these animals, which I am obliged to omit here, as it is scarcely susceptible of abridgment.

19. *Description and Analysis of a new Species of Lead Ore from India.* By Thomas Thomson, M.D. F.R.S., &c.—The specimen examined was brought by Dr. Heyne from Madras, where it is sold in the shops as a medicine. It approaches most nearly to galena in its appearance, though its characters are a good deal different from those of that mineral. I found it composed of

Lead	50·059
Copper	32·500
Iron	1·370
Sulphur	11·328
Loss	4·743
	100·000

The loss is too great; but I was not supplied with a sufficient quantity of the ore to repeat the analysis. From my mode of proceeding, I think it likely that the whole, or nearly the whole, of the loss was sulphur. In that case the ore would be a compound of

Sulphuret of lead	57·269
——— copper	40·850
——— iron	2·190
	100·309

Or, supposing the sulphuret of iron accidentally present, it is composed of an integrant particle of sulphuret of lead and two integrant particles of sulphuret of copper.

20. *Notice concerning the Structure of the Cells in the Combs of Bees and Wasps.* By Dr. Barclay.—It appears, from this communication, that the partitions between different cells in the combs of bees and wasps are all double; or in other words, that each cell is a distinct, separate, and in some measure an independent structure, agglutinated only to the neighbouring cells: and that when the agglutinating substance is destroyed, each cell may be entirely separated from the rest.

Article XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, the 12th of January, the remainder of Mr. Travers's paper on the way in which the eye accommodates itself to the sight of objects at different distances, was read. He considers the iris as muscular, and as connected with a kind of inner iris, or ring, which by its contraction increases the convexity of the lens. The eye he conceives is only acted upon by the stimulus of light. Hence he believes that the contraction of the iris is regulated entirely by the retina. The eye, in his opinion, is fitted naturally for viewing distant objects. When near objects are viewed the iris contracts, in consequence of which the inner ring acts upon the lens, and increases its convexity. The sensation of fatigue he supposes owing to the over-action of the external muscles of the eye, and not to any fatigue in the iris itself.

On Thursday, the 19th of January, a paper by Dr. Storer was read, giving an account of a well dug in Bridlington harbour, Yorkshire, within high water-mark. The bottom of the harbour is a bed of clay; through this they bored to the rock below; a tinned copper pipe was then put into the circular cavity, and the whole properly secured. The cavity was soon filled with pure water. When the tide rises to within about 50 inches of the mouth of this well, the fresh water begins to flow over, and the quantity flowing increases as the tide rises, and the flow continues till the tide sinks more than 50 inches below the mouth of the well. During storms, the water flows in waves, similar to the waves of the sea. Mr. Milne accounts for the flowing of this singular well in this way: the whole bay, he conceives, has a clay bottom. The water between the rock and this clay can flow out nowhere except at the termination of the clay, which is under the sea. As the tide rises, the obstruction to this mode of escape of the water will increase. Hence less will make its way below the clay, and of course it will rise and flow out at the top of the well.

At the same meeting a paper by Dr. Brewster was read, on the effect of pressure on transparent animal bodies, in causing them to polarize light. His first experiment was with a film of calf's-foot jelly. At first it produced no effect upon light; but as it became more and more firm it depolarized the light, at first at the edges, and at last throughout. The same thing was the case with a film of isinglass. When these films were subjected to pressure they depolarized light at first, and exhibited those complimentary colours which are peculiar to crystallized bodies.

LINNÆAN SOCIETY.

On Tuesday, the 17th of January, a paper by the Rev. Patrick Keith, on the epidermis of plants, was read. He gave an historical account of the different opinions entertained by vegetable physiologists respecting the epidermis. He himself considers it as composed of fibres rather than cells, an opinion adopted by some other persons. He gave an account of the epidermis of saffron, in which he found a peculiar structure. He observed pores likewise on the veins of leaves, where hitherto it has been supposed that they do not exist.

GEOLOGICAL SOCIETY.

In consequence of the great distance of the Editor from the place of meeting of the Geological Society, it has not been in his power for some time past to continue the regular details of the interesting papers read at their meetings. He now resumes these details from the time that they were interrupted.

March 4, 1814, a memoir, entitled, Observations on the Hill of Kinnoul, by Dr. Macculloch, V. Pr. G. S. was read.

The hill of Kinnoul, in the vicinity of the town of Perth, rises about 600 feet above the plain of the Tay. It is nearly a mile in length, and exhibits many abrupt faces in a state of constant ruin and depredation. It consists principally of floetz trap, sometimes in the state of black basalt, sometimes of a brownish red colour, but retaining the other characters of basalt; sometimes rendered porphyritic by the presence of numerous minute crystals of opake felspar; sometimes porphyritic and amygdaloidal; and sometimes simply amygdaloidal, with a base of basalt, often passing into wacke. The concretions of the amygdaloid are green earth or chlorite in small grains, and varying in structure from compact to completely scaly; calcareous spar, either single or mixed, with quartz or agate; quartz either common or amethystine; and agate and coloured and zoned chalcedony: sometimes the agate is partly stalactitical and partly zoned from green heliotrope without the red spots by which it is usually marked which also occurs in veins.

On the top of the great mass of trap is a bed of conglomerate, consisting of trap pebbles imbedded in a cement of the same nature. The most remarkable circumstance, however, in the hill of Kinnoul, is the variety of interesting junctions which it presents of the trap with grey-wacke-slate. These two rocks are intermixed and involved with the other in various ways, and the slate adjacent to the planes of contact is inflated and spongy, exhibiting a structure very analogous to that of the burnt micaceous schistus which is sometimes found in the walls of the vitrified forts, except that in the former the cavities are filled with calcareous spar.

The peculiarities in the structure of some of the agates, and in the trap and grey-wacke at the places of their junction, are in the opinion of the author of this paper, not to be accounted for by the exclusive agency either of fusion or of consolidation from aqueous solution.

A letter from Mr. Henry Sports, of Salisbury, on the formation of flint was also read. Certain of the flint nodules exhibit unquestionable marks of animal organization; and from this fact Mr. S. supposes that all flint has originated from sponges, alcyonia, and the spongeous zoophites, converted by some unknown process into silicious earth.

On the 18th of March and 1st of April, a paper by Nat. Wynch, Esq. on the geology of part of Northumberland and Durham was partly read.

Mr. W. begins his paper with a description of the magnesian lime-stone which makes its first appearance at Cullercoates, in Northumberland, and stretches in a S. W. direction between the rivers Tyne and Tees. At Whitley quarry, near Cullercoates, this formation may be seen resting on the coal strata; and in other parts the workings of adjacent collieries have been actually driven to a short distance under the lime-stone, although no sinkings begun in the lime-stone have ever been carried down as far as the coal. It is not, therefore, ascertained that the coal is of equal extent with the lime-stone; but unfortunately it is too well known that the coal has constantly proved to be excessively deteriorated where covered by the magnesian lime-stone. This formation is composed of strata of lime-stone of various qualities and appearance, such as white, brown, and fetid, very ferruginous, oolitic, &c. alternating with shale, and traversed by thin strings of galena. In the beds of the Tees occurs a red sand-stone, the geological relations of which are but little known. Sinkings have been made in it in different places to the depth of from 70 to 80 fathoms in search of coal, but wholly without success.

The coal formation rests upon the lead-mine measures, and is in part covered by the magnesian lime-stone. It is in the form of a trough, the extreme length of which from N. to S. is 58 miles; and its breadth from the sea-coast westwards is about 24 miles. The general inclination of the coal strata is one yard in 20, but subject to considerable local irregularities. The earthy beds which separate the coal seams from each other are potter's clay, slaty-clay in various states of induration, and sand-stone both massive and slaty. Of the latter beds some are quarried for flag-stone; and one, a buff-coloured fine-grained sand-stone, called the grind-stone sill, furnishes the celebrated Newcastle grind-stones. Thin beds and nodules of clay-iron-stone, the latter containing impressions of ferns and bivalve shells, occur in the shale or slaty-clay. A few dykes of basalt or green-stone intersect the coal formations. The best known is called Walker's Dyke. It is composed in Walker Colliery of two solid and parallel walls of green-stone of the thickness of three yards and six yards, with an interval of about 11 feet, composed of fragments of green-stone and sand-stone imbedded in blue slate. The dyke is perfectly vertical, and divides, but does not dislocate or heave the strata which it traverses. The coal on each side of it, to a distance of three to six yards, is converted into a hard cellular cinder, the cells of which are often occupied by calcareous spar and sulphur.

Fissures or slips are of frequent occurrence in this as in every other coal-field, and occasion much trouble and expense to the miner. Numerous mineral springs, containing more or less of common salt, occur in the whole of the coal-field.

April 15.—A communication from S. Solly, Esq. on the newer formations, particularly that of floetz trap, was read.

From the remarkable differences, notwithstanding their general resemblance, which prevail amongst the floetz trap, and other recent floetz rocks of the same species in different countries, Mr. Solly is induced to suppose that these are not parts of an universal formation, as is held by Werner and his pupils, but that they originate from local deposites. These local deposites are considered by the author of the paper as owing their peculiar structure and other characters partly to the influence of heat derived from the electric fluid, and partly to the action of crystalline polarity operating within them, while they were apparently in a quiescent state.

A paper by Dr. Berger on the geology of the North of Ireland was begun.

May 6.—The reading of Dr. Berger's paper on the geology of the North of Ireland was continued.

A paper entitled A Description of the Tunnel of Tavistock Canal through Morwel Down, in the County of Devon, by John Taylor, Esq. M.G.S. was read.

Morwel Down is a hill near Tavistock, and to the west of that place, which separates the valley of Tamar from that of the Tavy. The height of this hill is about 700 feet above the tide-way in the river Tamar. It is composed of schist (in the dialect of the country killas), and in the immediate vicinity of the mines which have recently been opened in that part of the county.

In 1803 an Act of Parliament was obtained for cutting a canal from the town of Tavistock to Morwelham, a quay on the river Tamar. An essential part of this plan was a tunnel through hard rock, about a mile and three quarters in length; passing through Morwel Down at an average depth, from the surface of about 400 feet, and in a direction calculated to cut through all the E. and W. or metalliferous veins that might traverse the hill. Of this important undertaking somewhat less than one-fourth remains to be performed; but the portion already executed has disclosed several important geological facts, which are detailed in this paper, and in the section and specimens by which it is accompanied.

Six beds or dykes have been cut through, the thickness of which varies from six to 26 fathoms. Their direction is inclined to that of the metalliferous veins, and they dip pretty uniformly to the North; they are composed of clay-porphyry, of another variety of porphyry, of quartz, and of quartz mixed with chlorite.

Several metallic veins, some of them already productive, and affording copper, and in a few instances tin, have also been discovered; they traverse all the strata, and exhibit a remarkable difference in their dip or under-lay on the two sides of the hill, those on the north side dipping to the north, and those on the south side dipping to the south.

May 20.—A communication from Lord Webb Seymour was read.

This paper is accompanied with explanatory drawings, and describes an instrument of his Lordship's invention called a clinometer, for the purpose of determining the position of the planes of stratification. The instrument itself had been previously presented to the Society.

A paper, accompanied with specimens, containing mineralogical remarks on part of the settlements of the Hudson's Bay Company, compiled from the reports of several observers, by the Earl of Selkirk, was read.

From Mr. Aulds it appears that the bed of Nelson river, seven miles below Hamborough Head, and not far from its mouth, presents several rapids, which are formed by flat strata of lime-stone, and in one instance by whin-stone (probably one of the trap rocks by which stratified lime-stone is so often accompanied). The effect of the ice floods in this river is very remarkable. The water charged with pieces of ice appears to be capable of detaching large blocks of lime-stone, and conveying them to a considerable distance. A deposite of this kind, the work of a single season, is described as forming a shoal 200 or 300 yards in length, and consisting of small stones covered by several hundred blocks of the same kind, weighing several tons each, and four feet and more in thickness.

There is reason to believe that the same lime-stone formation extends through all the country west of Lake Wimpie, as far as the rocky mountains. On the verge of these mountains coal has been observed; but the interior part of the ridge is described as composed of very hard rock, the stratification of which is nearly vertical.

A notice, accompanied by a very fine specimen of sulphate of strontian, by G. Cumberland, Esq. was read.

This specimen, with several smaller ones, was found in digging a vault in a burial-ground in Portland-street, Bristol. It is a nodule of considerable magnitude, pretty compact externally, and loosely filled up with crystals of larger size and more perfect figure than is usual in English specimens.

June 3.—A letter from Edward Smith, Esq. on the stream works at Pentowan, addressed to Dr. Wollaston, was read.

From the summit of Hensbarrow Hill to the sea-coast is about seven miles. A small river takes its principal rise in the above-mentioned hill, and after a rapid course, in many parts interrupted by cascades, falls into the sea near St. Austle. On hasty rains the rise of this stream is sudden, and often dangerous, but in dry seasons it is an inconsiderable rivulet. In the valley through which it flows are two stream-works, the upper and the lower.

The upper has been opened to the depth of 40 feet, and consists of six strata, or more properly alluvial beds. The uppermost, three feet thick, is the surface soil, with trees growing on it: the second is mud mixed with gravel, about 20 feet thick: the third is three feet thick, and consists of fragments of spar and killas; the fourth, five feet thick, consists of gravel and decomposed granite, the same as occurs at Hensbarrow Hill; the fifth bed, five feet thick, is gravel, covering large trunks and branches of oak: the sixth bed, five feet thick, is coarser gravel, intermixed with which are rounded pieces of tin-stone. This bed rests not on the rock, but on a kind of clay: it is, therefore, possible that there may be below other alluvial beds which have not as yet been examined.

The lower stream-work is about three quarters of a mile below the upper; and, like the latter, consists of a bed of tin-stone, five feet thick, in small rolled pieces, mixed with other fragments, and covered by about 54 feet of alluvial matter in several beds. Some of these are mere gravel; others are largely mixed with trees and other vegetable matter, among which have been found the horns of an animal of the stag kind; some large bones, supposed by Mr. Smith to be those of hippopotamus, and parts of two human skulls. There are also two beds of blue mud, containing marine shells.

A paper by Dr. Macculloch, being a supplement to a former communication of his on quartz rock, was read.

The object of the author in this paper is to confirm, by additional examples, his former position, that quartz rock is essentially composed of fragments and rolled pieces, and that it alternates with mica-slate and other rocks, which have hitherto been considered as a primitive.

June 17.—The reading of Mr. Wynch's paper on the mineralogy of Northumberland and Durham was concluded.

Below the *coal measures*, described in the former part of this paper, occur the *lead-mine measures*, which consist for the most part of lime-stone, sand-stone, and whin. The whin is a true trap rock, composed of felspar and hornblende, and is found in three different situations; first as dykes, or nearly vertical beds intersecting the regular strata; secondly, inclosed between the regular strata, and considered in general as one of them, although its thickness varies much more than that of the other strata, the *great whin sill* being in some places only six fathoms thick, and in others amounting to 20, and even 30 fathoms. Like the toad-stone of Derbyshire, it is not penetrated by any of the metallic veins that abound in the adjacent lime-stone. The third situation in which the whin occurs is in overlying masses with the external appearance of common coarse-grained basalt, and often shooting into regular columns of considerable magnitude.

The principal repository of lead is called the Great Lime-stone, about ten or eleven fathoms in thickness, and divided by indurated clay into three distinct strata. It is of a brownish or dark bluish-grey colour, and abounds in encrinites and bivalve shells. From an analysis of it by Sir H. Davy, it appears to be nearly pure carbonate of lime, containing neither magnesia nor alumina, and only about one per cent. of oxide of iron.

The *scar lime-stone* resembles the former in external characters, and the fossils which it contains; and is also divided into three strata, the aggregate thickness of which is about five fathoms.

The *u ehle shell lime-stone* is a thin bed, scarcely amounting to two feet, and is characterized by its containing bivalves, like oyster shells, four or five inches in diameter.

The next bed, called the Tyne Bottom Lime-stone, is an encrinal lime-stone divided into three strata, of the aggregate thickness of about 21 fathoms. It forms the bottom of the Alston Moor mining-field, but is nearly the uppermost bed at Dufton.

Below this are five other beds of mineral lime-stone, resembling the preceding.

Between the beds of lime-stone occur numerous beds of sand-stone, varying in structure from completely slaty to solid amorphous rock fit for mill-stones. Of these the brown ferruginous beds called *hazles* are found to be much more productive of lead than the others.

The common lead ore of this district is galena, holding silver in various proportions, from two ounces in the fother (21 cwt.) to 42 ounces. The general average, however, is about 12 ounces.

The *lead measures* rest upon the red sand-stone, and their total thickness is supposed to be about 457 fathoms. The metallic contents of the veins, besides several varieties of galena, are carbonate of lead, both crystallized, massive, and earthy, phosphate of lead, copper pyrites, azure copper ore, malachite, iron pyrites, sparry iron ore, blende, calamine, and siliciferous oxide of zinc. The vein-stones are quartz, several varieties of calcareous spar, coralloidal arragonite (flos ferri), pearl spar, crystallized and sandy fluor, witherite, and heavy spar.

The N. W. of Northumberland is occupied chiefly by the Cheviot Hills, which consist of various porphyritic and amygdaloidal rocks, accompanied on the side adjacent to the basset of the lead-mine measures by red sand-stone and grey-wacke-slate.

A communication from W. H. Pepys, Esq. Treas. G. S. was read.

This paper contains experiments to ascertain the composition of the sand tubes found at Drigg, from which it appears that the vitrified sand of which they are composed consists of 93 silex, 4·5 alumina and oxide of iron, 1 lime, 0·2 alkali.

Nov. 4.—A paper by Dr. Berger, on the whin dykes of the North of Ireland, was read.

The dykes of the district here mentioned are found at all the elevations from the level of the sea to the summit of Arragh, the highest mountain of Donnegal, being a range of 2220 feet. Where one occurs others are generally to be found in the immediate vicinity, and all of them running in parallel, or nearly parallel, directions to each other. This parallelism moreover obtains not only in adjacent dykes, but in almost all of this district, their general direction being N. W. and S. E., or perpendicular to that of the strata and the metallic veins, both of which are intersected by them.

The width of dykes varies considerably, from a few inches to several hundred feet; and upon the whole it appears that those which intersect primitive strata are narrower than those which intersect secondary strata. The substances which compose dykes being for the most part in a state of greater induration than the beds which they traverse, they may be perceived protruding above the general surface, either in the shape of vertical walls, in some instances 40 feet high, or of a mound composed of loose stones. The depth to which the dykes descend is wholly unknown; nor do they appear to become narrower in proportion as they descend.

The usual materials of dykes are trap and green-stone, with lydian-stone, flinty-slate, grey-stone, and wacke. Ferruginous sand-stone also occurs, but more rarely. The structure of a dyke presents columnar, or ovoid, or nearly rectangular concretions, laterally aggregated, and with their axis perpendicular to the walls; and the more compact the trap is the more apt it is to assume the columnar form.

Nov. 18.—The Secretary reported that a communication had been received from Wm. Phillips, Esq. M.G.S. on the granite of Cornwall.

A notice of some fossils found in the neighbourhood of Cambridge, by the Rev. I. Hailstone, Woodwardian Professor, M.G.S. was read.

The chalk hills of Cambridgeshire, where they terminate in the north, appear to rest on a bed of calcareous blue clay, provincially called *galt*. This bed, covered in some places with gravel, forms the extensive flat which connects the upper parts of the country with the fens.

The thickness of the galt bed, judging from the depth of the wells which have been sunk in it, is probably not less than 200 feet, and it is considered by the Professor as the lowest bed of the chalk formation. Above this bed lie several strata of a chalky stone, called *clunch*, which is an argillaceous lime-stone in great request, and of which large quantities are dug and burnt at Cherry Hinton, and other places in the county. The clunch contains no flint, but abounds in the common radiated pyrites. On some of the highest of the hills near Cambridge is found a deposite of gravel and loose stones in horizontal layers, which differs in so many respects from the gravel of the subjacent flat country that it must probably be considered as the deposite of another epoch. It consists principally of pale blue flint, enclosing numerous traces of ascidia, but also contains fragments belonging to the oolite formation of the neighbouring counties of Northampton, Rutland, and Lincolnshire, with occasional fragments of basaltic rocks.

The characteristic fossil of the clay or galt bed is that variety of belemnite described by Lister under the name of *lapis lyncurius*. There have also been found an obscure organic remain, considered by the Professor as a medusa or beroe; also a beautiful ammonite, a pentacrinite, and several species of bivalve shells. The mutilated remains of a species of fish have also been found; and charred wood is not uncommon among the animal remains.

Among the rarer fossils of the Cambridgeshire chalk, are fruit cones and linear leaves from Cherry Hinton.

The results of an analysis by Mr. Holme of three varieties of Cherry Hinton lime-stone are subjoined, from which it appears that the stone consists for the most part of carbonate of lime, but also contains from about four to nine per cent. of alumina, and a portion of silex not amounting to two per cent.

Two letters addressed to the Secretary from the Rev. Richard Hannah, jun. Chaplain to the troops at Plymouth, were read.

Some geologists having doubted or denied the existence of organic remains in the lime-stone of Plymouth, the object of Mr. Hannah's letters is to state that both madrepores and shells have recently been found by him in the quarries of Stonehouse Hill, and at the Dock-yard; a fact entirely corroborated by the specimens which Mr. H. has also transmitted for the cabinet of the Society.

Some observations on the interruptions or faults to which mineral veins are liable, contained in a letter addressed by Mr. I. Sadler to Ant. Carlisle, Esq. M.G.S. were read.

The cases solved in this paper are illustrated by diagrams, which renders it impossible to give an abridgment of them.

Dec. 2.—A letter from L. Horner, Esq. to the Secretary was read, describing certain specimens presented by him to the Society. They consist of two series: one exhibiting the junction of sandstone and of trap at the Kinnoul, near Perth; the other exhibiting a similar junction occurring in Salisbury Craig, near Edinburgh.

A paper by J. Macculloch, M.D. V.Pr.G.S. entitled Observations on the Mountain Cruachan, was read.

Cruachan, the highest of a cluster of mountains resembling each other in external form and geological composition, is situated in the east bank of the river Awe, between Loch Awe and Loch Etive. Instead of the rugged forms and rough faces that characterize the hills of mica-slate, situated to the south of this district, and forming the basins of Loch Lomond, Loch Lang, and Loch Fyne, it presents a more uniform flowing outline, a more complete covering of herbage, and a less serrated summit, which at the same time is strewed with heaps of fragments. On a near approach the rolled stones which abound in the beds of the torrents appear to be almost entirely composed of granite and porphyry; but the rock which appears *in situ* forming the lower part of the mountain in schist. This schist sometimes assumes the appearance of a compact mica-slate, but generally of clay-slate, the colour of which varies, from dark lead-grey to pale greenish-grey. Innumerable veins of granite and of syenitic granite of various sizes traverse the schist in all directions; sometimes passing clean through it, sometimes intermingled with it, or involving fragments of it. The schist is often excessively curved and contorted; and at the points of junction with the granite is either indurated and converted into lydian-stone, or appears insensibly melting, as it were, into the granite, and assuming the character of hornblende-slate.

The schist does not appear to reach above one-third of the entire height of the mountain; but nearly half of the remaining ascent is thickly strewed with loose rocky masses, and it is not till these have been surmounted that the crags and precipitous faces of granite begin to show themselves. This granite nearly resembles that of Carngorum, being composed of reddish felspar and white quartz in nearly equal proportions, with a small intermixture of mica: that which composes the summit strongly affects the magnetic needle.

Veins of compact felspar-porphyry make their appearance in great abundance in this granite: this breadth varies from three or four feet up to 40 or 50, or more; they are nearly vertical, but run horizontally in all directions. They traverse not merely the granite, but the schist likewise, and even the granite veins of the schist, differing remarkably, however, from these last in occasioning no disturbance in the strata through which they pass, nor intermixing in the least degree either with the granite or with the schist.

Besides the veins of granite and of porphyry, are veins of a grey trap rock, and others of a perfectly characterized basalt.

Similar appearances to these now described occur in the adjacent districts of Midlorn, of Argyle, and of Glenco.

WERNERIAN SOCIETY.

At the meeting of this Society on the 3d of December, there was read a communication from Captain Brown, of the Forfarshire Militia, describing five new species of shells, observed by him in Ireland. The description was accompanied by excellent drawings. A mineralogical communication from Mr. Giesecke was read, in which he described several new and rare minerals observed by him in West Greenland.

At the meeting of the 17th of December, the Secretary read a letter from Mr. Scott, at Ormiston, addressed to Lauder Dick, Esq.; containing a description of the junction of the transition and floetz rocks near Jedburgh, and mentioning the occurrence of granite in Roxburghshire.—Professor Jameson read the first part of a paper describing the mineralogy of the south district of Scotland, including a particular account of the rocks which occur in the neighbourhood of Dunbar, in East Lothian. The coast of Dunbar exhibits an alternation of the old red sand-stone with rocks of the trap formation; and interesting illustrations of the supposed chemical nature of sand-stone, and of the contemporaneous formation of masses of lime-stone in sand-stone, and of trap in sand-stone. At the same meeting, the first part of a memoir on the nature of the polar ice, by Mr. Scoresby, was read.—Specimens of sandstone, brought from Berskiming, in Ayrshire, by Mr. Miller, were exhibited. When cut into long thin bars they possess considerable flexibility, which is a good deal increased when the stone is wetted.

Article XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Phosphuret of Ammonia.*

M. Vogel has lately made a curious experiment in Paris. He put a piece of phosphorus into ammoniacal gas, and then exposed the phosphorus to the solar rays. A black matter was formed, which was conceived to be a combination of phosphorus and ammonia.

II. *New Expedition into Africa.*

We understand that the British Government are at present projecting a new expedition to explore the interior of Africa. They are to employ in this expedition the black regiment which has been formed on the west coast of Africa; and which, being inured to the climate, will constitute a better and more efficient guard to the travellers, than the same number of British troops. It is at present the most probable opinion, that the Niger, after running east for a considerable way, takes a southerly, and at last a westerly direction, and runs into the Atlantic on the coast of Guinea. It will be very singular if this opinion be confirmed. It would be of great importance if the travellers would carry with them a set of portable barometers, in order to determine the elevation of the country through which they pass. If we knew the elevation of the banks of the Niger, it would be easy to determine whether the opinion of the ancients, that it is a branch of the Nile, be possible or not. Nothing in Mungo Parke's travels would lead us to suppose that the elevation of the banks of this river is great. In that case it must either flow into an inland sea, or discharge itself into the Atlantic.

III. *Communication between the Blind and the Deaf and Dumb.*

Dr. Guillié, Director of the Royal Institution for the Blind, at Paris, by a method of his own invention, has been enabled to establish an immediate and perfect mode of intercourse between blind and deaf and dumb persons. These two species of unfortunate individuals, between whom nature appeared to have placed insuperable barriers, may, thanks to the ingenious author of this invaluable discovery, henceforward draw near and perfectly understand each other.

The first trial of this ingenious practice was made at a numerous and public meeting, held at Paris, the 26th of last August. A sentence was dictated to one of the deaf and dumb, named Masuen, a pupil of the Abbé Sicard, and by him communicated to one of the blind, who immediately repeated it in a loud voice; and he in his turn communicated to the deaf and dumb the sentence asked by the public, who instantly wrote it on a tablet.

We are assured that Dr. Guillié intends coming to London in the winter, to make an experiment of his practice before the Royal Society.

IV. *Swinestone at Building Hill, Durham.*

I am informed, by Mr. Sowerby, that some of the Building Hill stone retains its fœtid odour for years when scraped, while other specimens lose it very soon. This shows us that I was not mistaken when I called it Swinestone, on the spot. But the reason why some specimens soon lose their odour remains still to be given.

V. *On the Solar Spectrum.*

(To Dr. Thomson.)

SIR,

Since my last communication it has occurred to me, that if Dr. Herschel's position respecting the heating rays attendant on the prismatic spectrum stands in need of any confirmation, the well-conducted experiments of Mr. Leslie on radiant caloric substantiate them in a most remarkable manner. Mr. L. distinctly affirms that he found the greatest intensity, or, as I should call it, the *focus of heat*, by his reflectors, half an inch within, or nearer to the mirror than, the *optical focus*. As far as I can judge too, from the numbers stated by Mr. L., the progress of the differential thermometer from and towards the mirror, indicates the very ratio that the prismatic spectrum displays with regard to intensity of light. What then must be our conclusion, but that the reflector in this case performs an office similar to that of the prism?*

Now, Sir, may we not take advantage of this instrument to obtain a better knowledge of those rays termed deoxidating? In conformity to my theory *another focus* remains to be discovered, where muriate of silver would be found to blacken most readily; for I contend, that those rays are mere modifications of heat, or, to speak more fully, that they are rays of *latent* caloric, or the true phlogiston; while those which affect the thermometer are obvious or sensible caloric. With the greatest respect,

I am, Sir, Yours, &c.
David Huston.

VI. *Position of Port Jackson, New Holland.*

From the observations of Captain Flinders, it appears that Cattle Point, on the east side of Sidney Cove, is in latitude 33° 51′ 45·6″, south; longitude, 151° 11′ 49″, east.—*Flinder's Voyage*, I. 237.

VII. *Chinese Paste.*

The method of making paste in China is much more economical than the mode followed in this country. Were it universally adopted by trunk-makers, book-binders, and others, who use great quantities of paste, it would produce a very material saving of flour, which in years of scarcity might be of the greatest consequence. The following formula used in China was lately communicated to the Right Hon. Sir Joseph Banks, Bart., by a gentleman at present in Canton.—Mix together bullock's blood and quick-lime, in the proportion of one pound of the latter to 10lbs. of the former. It becomes a stiff jelly, in which state it is sold to the consumers, who beat it down with an addition of water into a state sufficiently fluid for use. At Canton it will keep five or six days in the hot weather and ten or twenty days in the cold. In Britain it would probably keep longer.

VIII. *Chinese Method of casting Sheet Lead.*

The Chinese method of casting sheet lead is very simple. Two large flat tiles are used for the purpose, covered on the inside with thick paper. The workman opening them a little at top, pours in a small quantity of melted lead, and pressing the tiles together with his foot, forms the sheet. A kind of resin called *dammer* is used to prevent the oxydizement of the metal. The tea canisters which have a crystallized appearance, and which at Canton are called *cho yeip*, or *bamboo leaf*, from their resemblance to that leaf, are formed from tin in the same way. They are not made in the neighbourhood of Canton.

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* As sound (*a*) is reflected as well as light and heat by a concave mirror, I have little doubt but that their analogy will be one day *perfectly* demonstrated.

(*a*) Query. Is the focus of sound at the same distance from the reflector as the focus of light?

IX. *Haüy's Crystallography.*

It gives us great pleasure to state, that Mr. Larkins, N° 14, Gee Street, Somer's Town, has commenced teaching the mathematical part of Haüy's theory of crystallization. It is scarcely necessary to observe, that without a correct knowledge of the mathematical principles from which this most beautiful theory has been deduced, very little progress can be made in the study of crystals. Mr. Larkins has likewise cut in wood very beautiful models of all the crystals described by Haüy in his System of Mineralogy. Complete sets of these models are sold at the rate of from thirteen pounds to fifteen pounds sterling, according to the kind of wood from which they are cut.

X. *Table of Temperatures.*

M. Guyton Morveau has been employed for many years in constructing a pyrometer of platinum to measure the higher degrees of heat. He was induced during his experiments to compare it with the mercurial thermometer, and with Wedgewood's pyrometer. The following table* exhibits the result of his experiments.

	Degrees of Wedgewood.	Degrees of Fahrenheit.
Mercury melts		−39°
Water freezes		+32
Water boils		212
Bismuth melts		475·99
Tin melts		512·48
Lead melts		611·98
Mercury boils	2°	642·75
Zinc melts	3	705·26
Antimony melts	7	955·23
Silver melts	22	1822·67
Copper melts	27	2205·15
Gold melts	32	2517·63
Welding point of iron	95	6508·88
Cast-iron melts	130	8696·24
Porcelain melts	155	9633·68
Manganese melts	160	10517·12
Macquer's furnace	165	10829·60
Fourneau a trois vents	170	11142·08
Malleable iron melts	175	11454·56
Nickel melts	175 + x	11454·56 + x
Platinum melts	175 + x	11454·56 + x

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* Annales de Chimie, xc. 236.

If I remember right, Wedgewood's clay pieces were baked in a red heat. If this be the case, it is impossible that the boiling point of mercury, which is below the commencement of Wedgewood's scale, can be represented by 2° of that scale. There is reason to believe from this, that Morveau was not in possession of the true clay pieces of Wedgewood. If so, his determination of the value of Wedgewood's degrees cannot be considered as correct. The melting point of tin is certainly stated too high in the preceding table. Mr. Crighton of Glasgow, whose accuracy is uncommon, found that this metal melts at 442°, and that it melts at a lower temperature than bismuth.

XI. *Saccharometer.—Thermometer.—Starch-Sugar.—Passage of Air through Tubes.*

(To Dr. Thomson.)

SIR,

I beg to make some observations on the queries in your *Annals*, of September, respecting the use of the saccharometer in brewing, which I had delayed.

According to the common acceptation of the word *strength*, in beer or ale, the saccharometer foretels it sufficiently near the truth, for *similar* processes in the *same* brewhouse; but it is not a sure guide for any dissimilarity in the mode of brewing or of drawing the extract: for then the constituents of malt are *not* often dissolved in the same proportion to each other; and, in all cases, the extracts are superior in value according to their priority. For, let the first, from the same parcel of malt, be reduced to the specific gravity of the last, and equal quantities of each will give a beer or ale very different in quality.

The quantity of alcohol increases during fermentation, as the specific gravity of the fermenting fluid diminishes, but in what ratio, no correct experiments have as yet ascertained. The term, designated *strength*, in malt liquors, depends, however, on many inexplicable circumstances; I fancy, from the variety in quality in different beers, made from equal extracts in every respect. I do not mean to allude to the mixture of any deleterious drugs, because I really believe the common brewers, in general, *do not*, and those extensive establishments in London, *cannot*, make use of any substitute for malt and hops. The gentleman, who makes those queries, is perfectly correct in asserting, that the common saccharometers do not give the quantity of solid matter contained in a barrel of wort; they express merely the difference of weight between that and a barrel of water. The former, however, can be ascertained nearly by multiplying the latter by 2, 7, according to my trials, which I hope agree with the experiments of this gentleman.

It is rather a disgrace to the present state of chemistry, that the chemical *brotherhood* of Europe should still suffer the variety in the thermometric scale to exist; I therefore, as an humble individual, beg to suggest, that a resolution, published by the leading chemists of England and France, to express temperature in future in none but *centigrade* degrees, would quickly become general, and form an universal language in this respect, to the expulsion of Fahrenheit's ridiculous scale, as well as the others, which occasion such unnecessary delay and difficulty in fixing the different degrees of heat in the memory, when running through chemical experiments in both the languages.

I may also perhaps be permitted to mention, to those who are making experiments on the conversion of starch into sugar, by Kirchoff's method, that if the boiling be performed by passing steam into the fluid, all risk of empyreuma may be avoided, desiccation prevented, (as the volume will continue any length of time nearly the same,) and the operation can be performed in porcelain, earthen ware, or even wood vessels.

Mr. Wilkinson, the celebrated Iron-master, could not produce any current of air through cast-iron pipes, (extending about a mile in length,) even to the distance of 600 feet from the impelling force, which was very great; yet the gas-light companies find no difficulty in this respect.—May I request an explanation of this anomaly from any of your correspondents? I. H.

XII. *Formation of Sal-Ammoniac in burning Bricks.*

(To Dr. Thomson.)

SIR,

In your Sketch of the latest Improvements in the Physical Sciences, under the division Salts, article Sal-ammoniac, you observe, " that the sand employed in making bricks probably contains common salt, derived from the sea water with which it is washed; and that this may be the source of the muriatic acid, which goes to the formation of the sal-ammoniac that sublimes during the burning of the bricks; but that if this be the case, nature employs, during this process, some method of decomposing common salt, at present unknown to manufacturers, &c."

In reply to this observation, I beg leave to remark, that as sulphate of ammonia generally accompanies the volatile products of burning coals, it is not improbable that this salt decomposes the common salt that adheres to the sand, and gives rise to the production of the muriate of ammonia in question.

I shall be much obliged to you to inform me, of some practicable method of separating manganese when in solution from the solution of a neutral salt. A minute portion of this metal communicates a permanent stain to a considerable quantity of any neutral sulphate that may happen to contain it.

I remain, Sir, your most humble servant,

Bristol, Jan. 14, 1815. E. T. I.

Article XV.

List of Patents.

Robert Salmon, Woburn, Bedfordshire; for improvements in the construction of machines for making hay. Aug. 22, 1814.

John Dickinson and **George Longman**, Nash Mills, Hertfordshire; for improvements in the said John Dickinson's patent machinery for manufacturing paper, and also a certain apparatus for separating the knots or lumps from paper or paper stuff. Aug. 24, 1814.

James Penny, of Low Nibthwaite, Lancaster, mechanic; and **Joseph Kendal**, same county, turner; for a new and improved principle of making pill and other small boxes. Sept. 8, 1814.

William Lister, Paddington, Middlesex, Esq.; for certain further improvements on an engine for separating corn or seeds from the straw and chaff. Sept. 21, 1814.

James Dobbs, Birmingham, Gentleman; for his improvements in the manufacturing of machines used for cutting and gathering in grain and produce arising from the earth, whereby much labour and expense are saved. Sept. 21, 1814.

Joseph Taylor and **Peter Taylor**, Manchester, machine-makers; for having invented and brought to perfection certain improvements in a loom to be used in weaving cotton, linen, worsted, silk, or other cloth or clothes, made of any two or more of the said materials. Sept. 21, 1814.

W. E. Sheffield, Somer's Town, Middlesex, Gentleman; for divers improvements in the working or manufacturing of copper and its compounds, and other metallic substances, or any or either of the same. Sept. 21, 1814.

W. Sampson, Acorn-street, London, millwright; for certain improvements for raising water. Oct. 3, 1814.

Abraham Shaw, Leicester, glazier's vice and diamond manufacturer; for his apparatus for the better cutting of window, plate, and sheet glass. Oct. 3, 1814.

Ambroise Firmin Didot, Holborn, London, Gentleman; for an improvement in the method of making types or characters to be used in the art of printing. Oct. 3, 1814.

R. Philips, Newbury, Bucks, engineer; for certain improvements in a plough. Oct. 5, 1814.

James Longhurst; for an Æolian organ or barrel organ with a self-acting swell. Nov. 1, 1814.

John Walters, Fenchurch-street, London; for certain improvements in the construction and fastening of frame timber or binds of ships or vessels, whether building or under repair. Nov. 7, 1814.

William Howard, Old Brentford, Middlesex; for improved apparatus for working the pumps on board ships, which may also be applied to churning, and various other useful purposes. Nov. 10, 1814.

ARTICLE XVI.

METEOROLOGICAL TABLE.

1814.	Wind.	Barometer.			Thermometer.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
12th Mo.										
Dec. 4		29·50	29·34	29·420	44	36	40·0			☾
5		29.94	29·50	29·720	41	33	37·0			
6		30·04	29·91	29·975	38	28	33·0			
7		30·04	29·60	29·820	45	33	39·0		—	
8	S	29·60	29·25	29·425	52	45	48·5		·44	
9	S W	29·70	29·25	29·475	52	25	33·5		6	
10		29·37	29·35	29·360	51	29	40·0	·11	·53	
11	W	29·63	29·37	29·500	54	48	51·0		·33	●
12	S W	29·65	29·50	29·575	56	53	44·5		·17	
13		29·72	29·50	29·610	56	41	48·5		3	
14	S W	29·56	29·54	29·550	53	42	47·5		7	
15	S W	29·68	29·40	29·540	55	43	49·0		3	
16	S W	29·92	29·40	29·660	55	37	46·0		·10	
17	S W	29·78	29·68	29·730	55	44	49·5		5	
18	S W	29·82	29·67	29·745	56	51	53·5	·33	2	
19	S W	30·05	29 67	29·860	55	30	42·5		·11	○
20	N	30·08	29·98	30·030	38	28	33·0			
21	N E	29·98	29·73	29·855	39	31	35·0			
22	E	29·73	29·16	29·595	37	32	34·5			
23	E	29·69	29·67	29·680	38	32	35·0			
24	E	29·67	29·62	29·645	33	29	31·0		—	
25	N E	29·61	29·58	29·595	33	30	31·5	·20	—	
26	N E	29·62	29·45	29·535	35	32	33·5		·16	☾
27	S E	29·45	28·94	29·195	41	35	38·0		—	
28	N E	29·70	28·94	29·320	40	33	36·5		1·14	
29	S E	29·78	29·70	29·740	43	34	38·5		2	
30	S	29·90	29·71	29·805	49	33	41·0		9	
31	E	30·18	29·90	30·040	44	31	37·5		—	
1st Mo.										
Jan. 1	N	30·43	30·18	30·305	42	30	36·0	4		
		30·43	28·94	29·665	56	25	40·13	0·68	3·35	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Twelfth Month.—4, 5, 6. Slight hoar frosts in the ponds: windy at intervals. 7. Occasional small rain. 8. The max. temp. of the last 24 hours this morning, with a hollow S. W. wind. An unsteady rain followed, of many hours' continuance, with much wind at S. 9. Windy: the barometer appears to have stood through the night at the min. noted. 10. Hoar frost: *Cirrostratus:* overcast, a. m.: after one, the day wet and stormy: much wind in the night. 11. Cloudy: rain before nine, and at intervals through the day. Though it was very cloudy, the bees came out in great numbers, as observed about the same time last year. 12. Temp. this morning 54°, with low driving clouds, and a gale at S. W. 13. Early this morning the wind was violent: it moderated about three p. m.: and the clouds, after a squall, assumed the *Cumulostratus.* Much water out in the marshes. 14. Red sun-rise: *Cirri* over the sky, with their extremities drawn out westward: p. m. it blew hard again from S. W. followed by rain at night. 15. a. m. Much wind and cloud. The millers, who have remarked that for two years past our atmosphere has been calmer than usual, will now probably complain of an excess of wind. The *Cumulostratus* appeared after three p. m. and the evening and night were calm. 16. Early this morning the wind rose again, and blew with great violence, with rain at intervals, till about three p. m., when the same change ensued as yesterday. 17. Overcast: windy: some rain p. m.: a gale through the night. 18. The tempestuous weather continues. 19. A shower about eight a. m., and again in the evening, after which light clouds were observed to pass the moon's disk with less and less velocity. 20. Hoar frost: a fine and nearly calm day: swarms of gnats in the air, and *gossamer* floating. The dew froze on the grass by half past four p. m.: there was a corona round the moon, and a *Stratus,* at night. 21. Hoar frost. 22. Dry air: steady breeze: *Cirrus,* passing to *Cirrostratus.* A lunar halo of large diameter. 23, 24. Cloudy: steady breeze. 25. Some snow in the night, followed by a little more in the day. 26. a. m. Snow more plentiful: a thaw. 27. Rain: sleet and snow: rain again. 28. The max. temp. early this morning: much rain. 29. Overcast sky. 30. Misty. 31. Hoar frost: large *Cirrus* clouds: a few drops of rain: p. m. *Nimbus.*

First Month, 1815.—1. Hoar frost: a rose-coloured sky, with *Cirrostratus* at sun-set.

RESULTS.

Winds in the fore part Westerly and violent: in the latter, Easterly and moderate: but in both attendant with rain: but one clear day in the period.

Barometer: Greatest height........................30·43 inches;
Least..............................28·94 inches;
Mean of the period ....29·665 inches.

Thermometer: Greatest height ....56°
Least......................................25°
Mean of the period ....40·13°

Evaporation, 0·68 inch. Rain, 3·35 inch.

Tottenham, *First Month,* 25, 1815. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

MARCH, 1815.

ARTICLE I.

A Biographical Account of David Rittenhouse, LL.D. F.R.S. late President of the American Philosophical Society.*

By Thomas Thomson, M.D. F.R.S.

THE Rittenhouse family from which our philosopher was descended emigrated from the province of Guelderland to New York, towards the end of the 17th century, when that state was in the possession of the Dutch. They soon removed to Germantown, in Pennsylvania, where they established the first paper-mill ever erected in America. Matthias, the youngest son of Nicholas Rittenhouse, our philosopher's grandfather, was born at Germantown, about eight miles from Philadelphia, in 1703. Having abandoned the occupation of a paper-maker when about 29 years of age, and two years after his father's death, he settled at Norriton, a farm which he had purchased of about 150 acres, and situated about 20 miles from Philadelphia. In the year 1727 he had married Elizabeth Williams, the daughter of Evan Williams, a native of Wales. This woman had been left an orphan, and was brought up by an old Gentleman in the neighbourhood called Richard Jones. She possessed a cheerful temper, and a mind uncommonly vigorous and comprehensive.†

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* The veneration in which this Gentleman was held in America, and the very high opinion entertained of his genius and scientific attainments, induce me to suppose that a short account of him will be acceptable to my readers. My information is derived from a biographical account of him published in Philadelphia in 1813, by his nephew, Mr. William Barton. I perused this book with much pleasure. The views which it gives of politics, philosophy, and religion, are excellent, and such as I scarcely expected from an American philosopher of the present day.

† Mr. Barton conceives that the abilities of David Rittenhouse were derived

By this wife Matthias Rittenhouse had four sons and six daughters. David, the eldest son, and the subject of this article, was born on the 8th of April, 1732. He was an infant when the family removed to Norriton. He was destined by his father for a farmer, and at the age of 14 was actually employed in ploughing his father's fields.

During this period, which continued for about five years, he appears to have been occupied chiefly in the study of mathematics, and in the pursuit of mechanical inventions. We do not know what books he possessed; but his brother Benjamin, who was often sent to call him in to dinner, often observed the fences at the ends of the furrows, and the plough handles themselves, chalked over with numerical calculations. In his 17th year he made a wooden clock of very ingenious workmanship; and soon afterwards he constructed one of the same materials that compose the common clock, and upon the same principles. It is obvious from this that he must have possessed the means of making himself acquainted with the principles of clock-making from books, since he had no other instructor; and possibly there might have been a clock in his father's house, which he would have it in his power to examine. Be this as it may, his success in constructing all the parts of the clock himself, and putting them together without any previous instruction, displayed an uncommon mechanical turn, which probably would not have appeared had he been placed in more favourable circumstances. He requested his father to allow him to devote the whole of his time to mechanics, and to set up the trade of a clock and mathematical instrument-maker. His father, after a good deal of hesitation, at last gave his consent. He erected in consequence a workshop on the side of a public road, and in the township of Norriton, after having made many implements of the trade with his own hands to supply the deficiency of such as were wanting in his purchased stock.

From the age of 19 to 25 Mr. Rittenhouse devoted the whole of his time to his trade and to his studies. The days were occupied with the former, and much of the nights with the latter. He rose very early in the morning, and did not go to bed till midnight, or even later. This intense application impaired his health, which had before been good. He was seized with a constant heat in the pit of the stomach, affecting a space not exceeding the size of half-a-guinea, attended at times with much pain. This malady never left him during the rest of his life.

In 1751, when Mr. Rittenhouse was 19 years of age, Mr. Thomas Barton, an Irish Gentleman of English extraction, opened a school in the neighbourhood of Mr. Matthias Rittenhouse. This

not from his father, but from this woman; and he is at some pains to show that abilities are often hereditary. For my own part I never knew a man of abilities whose mother had not been an uncommon woman. If we consider that the early part of education, the most important of all, depends almost solely on the mother, we need not be greatly surprised at this.

Gentleman had received a regular university education, and possessed considerable knowledge and abilities. He was two years older than Mr. Rittenhouse, and two years afterwards became his brother-in-law by marrying his sister. An intimate friendship took place between these two young men; and there can be no doubt that Mr. Barton must have been of considerable use in forwarding the education of Mr. Rittenhouse, both by supplying him with books, and by instructing him in several branches of knowledge, to which previously he could not have the means of paying any attention; but we neither know the course of study that Mr. Rittenhouse pursued, nor the books which he used. He appears early to have acquired a very considerable knowledge of mathematics and astronomy, and to have perused Newton's Principia partly in the English translation and partly in the original. Mr. Barton says that he discovered the method of fluctions without knowing that it had been previously discovered by Newton; but such ignorance in the year 1751, more than half a century after the discovery was publicly known in Europe, seems hardly conceivable. Whatever mathematical books he studied must have alluded to a method at that time followed by every mathematician; or supposing, which seems hardly credible, that all the books in his possession had been printed before the discovery of fluctions, still Mr. Barton, who had been educated at Trinity College, Dublin, could not possibly be ignorant of the discovery of fluctions. Indeed, in the second edition of the Principia, there is a kind of discussion on the subject by Newton himself. Mr. Barton then, I conceive, must have been misinformed upon this subject.

Astronomy was Mr. Rittenhouse's darling pursuit; and we have the strongest proofs that his knowledge of this science was profound, and his skill as a practical astronomer very great. He acquired considerable dexterity in making astronomical instruments, especially telescopes and clocks. The principles by means of which the pendulums of these clocks were kept always at the same length are curious, and deserve the attention of astronomers and mathematical instrument-makers.

But though neither the books which he perused, nor the order of his studies, be known, yet we are acquainted with the particular circumstance which first turned his attention to mechanics and mathematics. He had a maternal uncle, David Williams, a carpenter, who had a mathematical turn: this man died when David Rittenhouse was a boy, and his tools and books, consisting of some treatises on geometry and some manuscript calculations, fell into the possession of his mother; to these books, &c. David had free access, and they were probably the only books of any value within his reach. Hence we need not be surprised that a young man of a vigorous understanding soon acquired a taste for mathematics, and learned to handle the tools of his uncle.

The great mathematical and astronomical skill of Mr. Rittenhouse was soon discovered by Mr. Barton, who had taken orders, and was settled as a clergyman in an episcopal congregation in Philadelphia. He communicated the fact to Dr. Smith, Provost of the College of Philadelphia: a friendship soon took place between them, and Dr. Smith was at great pains to draw him into public notice.

In 1763 Mr. Rittenhouse was employed by Mr. Peters, Provincial Secretary to the Governor of Pennsylvania, to determine part of the limits between Maryland and Pennsylvania, which had long been a subject of dispute between Lord Baltimore and Mr. Penn, the proprietors of the respective provinces. This laborious task he performed to the satisfaction of the parties concerned.

In 1766 Mr. Rittenhouse married Eleanor Colston, daughter of Bernard Colston, a reputable farmer in the neighbourhood. She belonged to the society of Quakers. Mr. Rittenhouse had been brought up an Anabaptist; but he never declared himself a member of any particular church. The marriage was solemnized at Norriton by his brother-in-law, Mr. Barton. His father had previously resigned to him his house and farm of Norriton, having removed with his family to a house that he had built on his place in Worcester township. At Norriton Mr. Rittenhouse remained for four years; during which period his reputation as a mechanic and an astronomer was very much increased by two circumstances, which deserve to be particularly mentioned. The first was the construction of an orrery, much more complete than any thing of the kind that had been previously contrived. It exhibited the true motions of the different planets and satellites with the greatest exactness, so as to show their place in the heavens, and their relative position with respect to each other without any sensible error for thousands of years to come. The relative position of these bodies for any particular time could be determined by turning a winch till an index on a dial plate pointed out the time wanted. Before he attempted the construction of this expensive machine Mr. Barton, his brother-in-law, bound himself to reimburse him for the expense, provided he could not get it sold; but as soon as the machine was completed, there was a competition for it between the College of Princeton, New Jersey, and the College of Philadelphia. The orrery was disposed of to the College of Princeton for 300*l.*; and he made another within the year for the College of Philadelphia.

The other circumstance was of still greater importance: it was the observation of the transit of Venus in 1769. This transit, being of rare occurrence, and being the best means of determining the parallax of the sun, was eagerly expected by astronomers, and had attracted the particular attention of almost all the crowned heads in Europe. Philadelphia being a proper station for observing this phenomenon, the opportunity was laid hold of by the Philosophical Society of that city, and three committees were appointed to make the observation in three different places: one of these was at the observatory constructed by Mr. Rittenhouse at Norriton. The committee appointed to make the observation in this place were

Mr. Rittenhouse, Dr. Smith, Mr. Lukens, and Mr. Sellers. Every thing was properly prepared by Mr. Rittenhouse. The sun rose and set on the day of the transit without a cloud. The observation was made in the most complete manner. An account of it was published in the first volume of the Transactions of the American Society. It was allowed by astronomers to be very complete, and served to raise the reputation of Mr. Rittenhouse in Europe. He was henceforth considered as a consummate astronomer.

On the 17th of November, 1767, the College of Philadelphia bestowed upon Mr. Rittenhouse the honorary degree of Master of Arts. In 1770 he took up his residence in Philadelphia. About the same time he lost his wife, who left him two daughters, both at that time infants. In 1771 he was elected one of the Secretaries to the Philosophical Society. About this time the Assembly of Pennsylvania voted him 300*l.* chiefly on account of the ingenuity which he had displayed in the construction of the orrery.

It was at this time that the disputes between the British Government and the American colonies, which terminated in the revolutionary war, began. No further attention could be paid to science or literature; politics alone filled the minds of every one. Mr. Barton assures us that at the commencement of the dispute the whole American population was loyal, and that it was with the utmost reluctance they were brought to a declaration of independence; but I cannot, for my part, adopt this opinion. If we consider the great number of emigrants that went to America during the troubles in Charles the First's reign; that these emigrants were all puritans who possessed the most lofty ideas of liberty and independence, and were in fact republicans in their hearts; we cannot avoid concluding that these sentiments, for which they had left their native country, and suffered all the hardships to which they were exposed in the New World, would be cherished by them with particular veneration, would be inculcated upon their children, and would become the distinguishing characteristic of the colony. Hence I conceive that republican principles were very generally entertained in America, especially in New England, where the dispute first began. The colonists were of opinion that the whole expense of defending them should fall upon the mother country, and that the British Parliament had no right to lay taxes on them till they put them in every respect upon a footing with the inhabitants of Great Britain. In this opinion I think they were right. It was fortunate for Great Britain that the dispute terminated as it did; perhaps it was equally fortunate for America, though a sufficient period has not yet elapsed to enable us to judge correctly.

Mr. Rittenhouse took the side of the colonists with great keenness; while his brother-in-law, Mr. Barton, embraced that of the mother country. This produced a separation. Mr. Barton went to New York, where he died in 1780. He left all his family behind him in Philadelphia. In 1776 Mr. Rittenhouse was chosen State Treasurer of Pennsylvania, a situation which he filled for 13 years.

The profits were so small as not to enable him to keep a clerk, and the finances of the country so embarrassed as to render the office far from agreeable. In 1777 the British army occupied Philadelphia. The State Legislature went to Lancaster, and Mr. Rittenhouse accompanied them, leaving his wife and family (for he had married again) at Norriton. A Council of Safety, consisting of twelve persons, of whom Mr. Rittenhouse was one, was appointed with despotic powers; but it does not appear that they ever exercised them. When the British army evacuated Philadelphia, in 1778, Mr. Rittenhouse returned to that city, and continued to reside in it during the rest of his life.

In 1779 he was appointed one of the commissioners to settle the boundary between Pennsylvania and Virginia, which had been disputed for so long a period with so much eagerness. The business was finally settled in 1784, Mr. Rittenhouse and his associates having marked out the whole boundary line with the greatest accuracy. On the 10th of March, 1780, Mr. Rittenhouse was elected, by the General Assembly of Philadelphia, a Trustee of the Loan Office of the State. In 1782 he was chosen a Fellow of the Academy of Arts and Sciences of Boston. In 1786 he was appointed a commissioner to determine the exact boundary between the State of Massachusetts and New York. In 1789 the degree of Doctor of Laws was conferred upon him by the College of New Jersey. The same year he resigned the office of Treasurer of the State, which he had held for 13 years, having been unanimously elected to it every year by the vote of the Assembly. In 1790 he was elected Vice President of the American Philosophical Society; and the next year, in consequence of the death of Dr. Franklin, he was chosen President of that learned Body, a situation which he held till his death.

He was employed by Government, on various occasions, in the construction of roads, and in rendering rivers navigable, or in improving their navigation; and in 1792 he received the very important appointment of Director of the American Mint, a situation which he held till 1795, when he resigned it.

The French Revolution had been going on for some years, and it is not surprising that it was generally approved of by the inhabitants of America: so strong were their prejudices, indeed, that even the destruction of the King of France, and of the whole family, the bloody despotism of Robespierre, and the fanatical madness of the pretended philosophers who abolished by law the Christian religion, the belief in the existence of God, and of a future state, did not open their eyes, nor make them sensible of the desperate evils to Europe, morality, and civilization, with which that ill-starred revolution was pregnant. A democratic society was established at Philadelphia in 1793 in imitation of the Jacobin Club of Paris, and Mr. Rittenhouse was elected the President. The bad tendency of this society was soon evident; and it became the source of many political evils, particularly of an insurrection in the west of Pennsylvania against the excise laws. Mr. Barton has endeavoured to vindicate Mr. Rittenhouse for his connection with this Society; but his vindication is far from satisfactory. He did not attend their meetings; but as he retained the place of President, this must be considered as a direct avowal that he approved of their proceedings; for had he disapproved of them, it is quite obvious that he was bound as an honest man to throw up the office. Now as Mr. Barton does not say that any resignation took place, it is pretty clear that he retained his situation to the last. How far his being President of the democratic society of Philadelphia can be considered as approving of the enormities committed in France, and the desperate spirit of immorality displayed by the successive demagogues of that unhappy country, I cannot say; because I do not know the peculiar creed which was maintained and propagated by the democratic society of Philadelphia. That a republican should view the French Revolution with a favourable eye, in spite of the enormities which disgraced it, I can easily conceive; but that a philosopher, and a disciple of Newton, should view with complacency the unblushing propagation of atheism and immorality, is what I cannot bring myself to believe; nor that a Christian, and a man of virtue, like Mr. Rittenhouse, should rejoice at the destruction of the Christian religion, and all the institutions for education in one of the finest countries of Europe.

Dr. Rittenhouse was elected a foreign member of the Royal Society on the 16th of April, 1795; but he enjoyed this honour only a short time. His constitution was worn out, though he had not attained any extraordinary age; and he died on the 26th of June, aged 64 years. The account of his death by his nephew, Dr. Smith Barton, who attended him in his last illness, is so simple and interesting that I cannot avoid laying it before my readers :—

" The last visit I ever received from Mr. Rittenhouse was about the middle of June, 1796. He called at my humble habitation in Fifth-street, to inquire about my health, and to learn from me the result of the experiments and inquiries in which he knew I was at that time engaged, concerning the mode of generation and gestation of our opossum, an animal to whose economy and manners he had himself paid some attention, and whose history he justly considered as one of the most interesting in the whole range of zoology.

" It was on this occasion that our excellent friend first informed me that he had received a diploma from the Royal Society. He observed, with a tone of voice, and with a certain expression of countenance, which were not calculated to afford me any pleasure, that a few years ago such a mark of respect from that illustrious body would have been received by him with pleasure and with pride.

" In fact, Mr. Rittenhouse, now and for some months past, was strongly impressed with the idea that his career of usefulness and virtue was nearly at an end. He had several times during the preceding part of the spring and summer, intimated to me (and doubtless to others of his friends) his impressions on this head. In what precise condition of his system, whether physical or intellectual, these impressions were founded, I have only been able to form a distant and unsatisfactory conjecture.

"A few days after this interview, viz. on the 22d of June, I was sent for to visit Mr. Rittenhouse. I found him in his garden, where he loved to walk, and soon learned that he laboured under a severe attack of cholera, accompanied, however, with more fever than we generally find with this disease; and with a great increase of that violent pain and sense of oppression at the region of his stomach, to which he had been subject for at least thirty years. Notwithstanding his age, the debility of his system, and the unfavourable state of the season, I ventured to flatter myself that the attack would not prove mortal. On the following day, however, finding him no better, but rather worse, I requested permission to call in the aid of another physician; and having mentioned the name of Dr. Adam Kuhn, that Gentleman accordingly visited our friend in company with me during the remainder of his illness.

"His febrile symptoms being very urgent, it was thought necessary to bleed our patient; and notwithstanding his great and habitual repugnance to the practice on former occasions, he now readily consented to the operation, on condition that I should perform it myself. The blood which was drawn exhibited a pretty strong inflammatory crust, and the operation seemed to give him a temporary relief from his pain. Soon after this his strength gradually declined; and on the third day of his illness it was but too obvious that our illustrious relative was soon to be separated from his friends. He expired without a struggle, and in the calmest manner, ten minutes before two o'clock on the morning of Sunday the 26th, in the presence of his youngest daughter, Mrs. Waters, and myself. His excellent wife, who had ever been assiduous in her attentions on her husband, both in sickness and in health, had retired from his chamber about two hours before, unable to support the awful scene of expiring genius and virtue.

"There can be no doubt, I think, that Mr. Rittenhouse, from the first invasion of his disease, or at least from the day when he was confined to his bed or room, entertained but little hopes of his recovery. He signed his will in my presence. He discovered no more solicitude about his situation than it is decorous and proper in every good or great man to feel when in a similar situation. During the greater part of his illness he manifested the most happy temperament of mind: and it was only in the last hour or two of his life that his powerful intellects were disturbed by a mild delirium. About eight hours before he died, the pain in the region of his stomach being unusually severe, a poultice composed of meal and laudanum was applied to the part. In less than two hours after the application I called to see him, and upon asking him if he did not feel easier, he calmly answered in these memorable words, which it is impossible for me to forget, for they were the last he ever distinctly uttered, and they make us acquainted with the two most important features of his religious creed,—' Yes, you have made the way to God easier.'

" Such were the dying words, as it were, of our illustrious relative and friend. He was dear to us both, to all his relatives and friends, and to his country. To me, let me add, he was *peculiarly* dear. The most happy and profitable hours of my life were passed in the society of this virtuous man. I followed his footsteps in the wilderness of our country, where he was the first to carry the telescope, and to mark the motions and positions of the planets. In the bosom of his family I listened to his lessons, as an humble disciple of Socrates or Plato. Science mixed with virtue was ever inculcated from his lips; but to me Mr. Rittenhouse was more than a friend and preceptor. He was a father and supporter. He laid the foundation of what little prosperity in life I now, or may in future, enjoy: and if it shall ever be my fortune, either by my labours or my zeal to advance the progress of science, or to reflect any honour upon my country, I should be the most ungrateful of men if I did not acknowledge and wish it to be known that it was DAVID RITTENHOUSE who enabled me to be useful."

That the character and dispositions of Dr. Rittenhouse must have been excellent, is obvious from the high veneration in which he was held by all his countrymen, and by the annual and unanimous vote of the General Assembly of Pennsylvania in his favour for 13 years, at a period when they were divided into two most furious factions. That his honour and integrity were pure and unsullied we require no further proof than this: he led in Philadelphia a retired and sober life, never launching out into any extravagance, and indeed holding luxury in the utmost abhorrence; yet all the property which he ever acquired cost him no more than 13,525*l.*; though he held for many years public offices of the most important kind, some of them lucrative; and one, Treasurer to the State, in a time of public danger and distress, in which a man destitute of principles might have realized an immense fortune. Mr. Barton has brought the most satisfactory evidence that he was a sincere believer in the Christian religion, that he had studied its evidences, and satisfied himself of their force, though he seems to have reckoned as of no consequence the peculiar dogmas which distinguish the different Protestant sects which swarm in America. His liberality and benevolence were in all cases conspicuous. He advanced a considerable sum of money to enable the American Philosophical Society to discharge a debt which they had contracted. In 1793, when the yellow fever raged in Philadelphia, he employed Dr. Barton to attend several poor families in his neighbourhood who had contracted the disease. He supported, for some time, an Italian statuary, who had come to Philadelphia in quest of employment, but had been unsuccessful. Finally, he was, during the whole of his life, a violent and declared enemy to the slave-trade.

As to his works, he has left but few behind him. Nor shall we be surprised at this, if we consider the country in which he spent his life, the difficulties with which he had to struggle in the early part of his career, and the arduous contest which, during a period of 12 years, drew the whole attention of the Americans to politics, and which left behind it such a deterioration of moral character, such a state of agitation and discontent, as must long prevent any great advancement of the sciences in that boisterous and unsettled republic. Mr. Rittenhouse left behind him 22 papers, all printed in the four volumes of the American Philosophical Society's Transactions, which have been already published. These papers are chiefly astronomical; and the most important of them all is the account of the transit of Venus in 1769, printed in the first volume, and drawn up by Dr. Smith.

ARTICLE II.

Experiments for determining the Draught of Carriages, with and without Springs. Extracted from an Essay on the Construction of Roads and Carriages. By R. L. Edgeworth, Esq. F.R.S. M.R.I.A. and Civil Engineer.

THEORY shows that whilst the wheels of a carriage pass over an obstacle the load on the carriage must rise along with the wheels, unless it is supported by springs; but that if the load is hung upon springs, whilst the carriage-wheels tend to throw the load upwards, as they rise suddenly over an obstacle, the springs will bend, because they are opposed, not only by the weight, but by the *vis inertiæ* of the load acting downwards; and the load will consequently not be thrown up *suddenly* so high, as if there were no springs; for the weight cannot be thrown upwards instantly; it requires a certain time to throw any weight upwards through a given space, and it is well known that in all cases this time must be equal to that in which a body would fall through the same space. So that making allowance for the imperfection of springs, it is easy to calculate their utility in lessening the draught of a carriage over an obstacle, if the height of the obstacle, the velocity of the carriage, the height of its wheels, and of the load which rests on springs, be known.

Upon subjects of this sort, which are of such universal concern, the best possible proofs should be given of the truth of whatever is proposed for general adoption, particularly where popular prejudice is directly in opposition to what is advanced. Let the accuracy of models, and their resemblance to *reality*, be ever so great, the mind still requires what is real. The ploughman will not be convinced by experiments made on the *models* of ploughs, nor the waggoner or coachman by experiments on the *models* of coaches or waggons. It has been hitherto, therefore, a desideratum in mechanics to discover means of ascertaining precisely the power exerted by animals in drawing instruments of agriculture, as well as what is requisite to draw wheel carriages.

A machine for this purpose was presented to the Society for the Encouragement of Arts and Manufactures in 1771. It consisted of levers acting upon a spiral spring, in such a manner as to move an index that went through a considerable space. But as the power of the horse, and the resistance of the machine, varies continually; and as this machine showed only the *greatest* exertion of the power, without giving the sum of all its efforts, it was not satisfactory. It is possible to construct a machine upon this principle that will give the amount of the time and force employed in trials of this sort; but more simple and satisfactory means have been discovered.

In page 50 of the first Report for May, 1808, of the Committee of the Highways, I have spoken of a perfect method of ascertaining the comparative force necessary to draw any two carriages. It appears to me to be my *duty* * to describe this contrivance in the present paper.

To elucidate the principle of this apparatus, two post-chaises were selected, the weights of which were nearly the same, their wheels were nearly of the same size, and they were alike in their general construction. To determine which of these could be drawn over the same obstacles with the greatest ease, one end of a rope 60 yards long was fastened to the splinter-bar of one of the chaises, and the other end of it was fastened to the splinter-bar of the other carriage. This rope had been previously made to pass round a light pulley six feet six inches diameter, which was placed horizontally, by means of a proper frame, on a two-wheeled carriage.

This pulley was sufficiently large to permit the carriages which were to be compared to run in a parallel direction at a sufficient distance from each other.

The foremost carriage was drawn forward by horses, and the two chaises were obliged to follow it. As the chaises were nearly of the same weight, they kept for some time together; but when either of them was retarded, it fell behind the other. An inconsiderable difference in the goodness of the parts of the road on which either carriage ran became sensible by the retardation of that carriage which ran upon the worst road.

If the machine carrying the pulley be drawn forward, the two other carriages must follow it; and if they are of equal weight, and equally well constructed, they must move on together, as they are drawn by the carriage with the pulley, provided the part of the road on which each of them moves be equally smooth and good; but if either of the carriages that are to be compared is inferior in construction to the other, it will not keep pace with that of a better construction, but it will in a short time be left behind, till at last the better carriage will overtake the machine which carries the pulley. Now if the carriages are replaced in their former situation, that which appeared to be the best may have additional loading put upon it by degrees till it nearly keeps pace with the inferior carriage; so that after a sufficient number of trials, the advantage of one over the other may be determined by weight.

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* This essay was composed at the desire of the Chairman of the Committee of the House of Commons.

This is in fact weighing the draught of one carriage against the draught of another; and thus, taking any one carriage as a standard, the comparative advantage of any number of carriages, ploughs, or implements to be drawn by horses, may be accurately ascertained.

By this apparatus a cart with wooden springs was tried against a common cart: both were in the first place weighed, and the lightest had as much loading put into it as made it equal in weight to the other. They were then fastened to the ends of a rope which went round a pulley which was mounted, as above described, on another carriage. After a number of trials, on a road which was neither very good nor very bad, it was fairly ascertained that the cart on wooden springs surpassed a common cart of nearly the same form, and with wheels of nearly the same size; and that it carried more than one-fifth greater loading than the common cart; that is to say, the spring cart, loaded with seven men and a boy, kept pace with a common cart loaded with six men, whose weights compared were in the proportion above-mentioned.

To prevent any mistake that might arise from the difference of roughness in the different paths on which the carts ran, they were placed alternately at each end of the road, without any material difference being observed.*

Distance from workmen skilled in working steel springs prevented a comparison being made between them and wooden springs.

Having thus stated the result of an inquiry, which appears to me of extensive utility, I beg leave to point out in general,

That if one horse out of five which are now employed in drawing heavy burdens could be spared, the saving to the nation would be (according to the calculations of Mr. Ward, in his excellent paper printed in the Third Report for 1809, of the House of Commons' Committee upon Broad-Wheels and Roads) nearly three millions sterling.

That as the advantage of springs must be inconsiderable upon smooth, soft, and sandy roads, their utility may perhaps be confined to one-tenth part of the roads in England, still they may save nearly 300,000*l.* annually to the nation, with all the advantages arising from the greater supply of human food, which must arise from the saving of land now appropriated to the maintaining of horses. Supposing the saving to be only one-half or one-third of this sum, surely it is a great consideration in political economy.

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* A similar experiment to what is above described was tried on a gravel walk with models, and nearly the same results were produced.

It may also be observed, that the wear and tear of carriages on rough roads will be considerably diminished by the use of springs:

That the carriages with springs may be made much lighter than those without them, and that the weight so saved may be part of the loading of the carriage, instead of being uselessly a part of the carriage itself:

That by such carriages the roads will be less injured than they are at present, and that the thill-horse, though drawing heavier loads, will not be liable to the violent succussions to which it is now exposed in bad roads.

If this paper should in any degree obtain the attention of the Board of Agriculture, it may perhaps conduce to promote the adoption of a scheme which appears to me of the utmost national importance. I allude to a proposal, which I have elsewhere made, of carrying on publicly and daily for some months in the vicinity of the metropolis a system of large experiments with real carriages on a real road.

For this purpose a piece of a common near a great road should be fenced off, and there a quarter of a mile of road should be constructed on the best principles. And on this road, during two months at least, carriages upon the most approved construction should constantly be employed. Their number should be such that the traffic on the experimental road should equal that which is carried on upon the common road with which it is compared.

Beside this, part of the enclosure should be appropriated to experiments upon large carriages of different construction, with and without springs, in the manner above described.

These last-mentioned experiments might be repeated once a week for the period above-mentioned, so that the philosopher, the legislator, the farmer, the manufacturer, the coach-maker, the wheel-wright, the coachman, and the carter, might satisfy himself by the conviction of his own senses, observation, and understanding, of the true practical result of all that has been said, written, and tried, upon the subject of wheel carriages.

Such a large and unequivocal exposition of the truth would put an end to many a vain and interested project. It would be the result of real patriotism—of that genuine English patriotism which generously promotes what is essential to the economical interests of the state. Here nothing is promised but what every man in England must wish to have accomplished; and a scheme is proposed which no adventurer can turn to private advantage.

ARTICLE III.

Experiments to determine the Definite Proportions in which the Elements of Organic Nature are combined. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

III. *Analysis of the Ternary Oxides.*

(*Continued from* p. 101.)

5. *Acetic Acid.*—Among my experiments on the water of combination of the salts, I gave two analyses of acetates; namely, acetate of lime and acetate of soda, from which I concluded that 100 of acetic acid neutralize a quantity of base containing from 15·71 to 15·89 of oxygen.

I have thought it necessary to repeat these experiments with still greater accuracy. I held acetate of lime for several hours in the highest temperature to which it could be exposed without risking decomposition: 10 parts of this salt burnt in a platinum crucible to the state of pure quick-lime left 3·54 of residue. Hence acetate of lime is composed of

Acetic acid	64·6	100
Lime	35·4	54·8
	100·0	

These 54·8 parts of lime contain 15·43 of oxygen. I took very pure crystals of acetate of lead, reduced them to powder, and put them into a vacuum with sulphuric acid, in which I left them for 48 hours. I chose this method because acetate of lead undergoes a slight decomposition in the open air, attracting carbonic acid, and letting go a little acetic acid; and when heat is applied, a part of the acetic acid flies off along with the water. In three different experiments, the loss of weight was 14·29, 14·32, and 14·5 per cent. These variations depend upon the impossibility of determining when all the humidity mechanically retained is dissipated, and the salt begins to effloresce. The salt effloresced in this manner, when heated in a retort, gave out neither water nor acetic acid (I mean before it began to be decomposed); doubtless because acetic, like many other acids, is unable to exist in a separate state when entirely deprived of water or some other oxide with which it may combine.

A hundred parts of effloresced acetate, analyzed by combustion, and by driving off the acetic acid by means of nitric acid, and decomposing the nitrate by heat, produced in different experiments from 68·5 to 68·52 of oxide of lead. Hence it is composed of

Acetic acid	26·97	31·48	100·000
Oxide of lead	58·71	68·52	217·662
Water	14·32		53·140
	100·00	100·00	

But 217·662 of oxide of lead contain 15·56 of oxygen, which differs little from the analysis of acetate of lime. The oxygen of 53·14 water is 46·9; but 15·56 × 3 = 46·68; that is to say, that the water contains three times as much oxygen as the base.

To find the number of volumes of oxygen in acetic acid, I examined the subacetate of lead. It is known that acetate of lead digested on oxide of lead dissolves a portion of it, and forms a soluble subacetate; while a portion of the oxide of lead is converted into a white voluminous powder, little soluble in water, and constituting a subacetate with a greater proportion of base.

The soluble subacetate attracts carbonic acid from the atmosphere with avidity, and cannot be concentrated in open vessels. To obtain it in a dry form, I exposed a solution of subacetate of lead in a vacuum with sulphuric acid. When half the liquid was evaporated, I found a great quantity of a white matter, not at all crystallized, deposited at the bottom. I removed this matter from the liquid, washed it in alcohol, and then dried it in a vacuum along with sulphuric acid.

The subacetate thus prepared, when heated in a retort, gives no symptoms of containing moisture, and of course does not contain any combined water. Analyzed by combustion, it gives 86·77 per cent. of oxide of lead.

The liquid from which this salt had been deposited, when mixed with alcohol, lets fall a new portion of the salt; which, being analyzed, gave the same result as the preceding. Hence this subsalt is composed of

Acetic acid	13·23	100
Oxide of lead	86·77	656
	100·00	

But 217·66 × 3 = 652·98, which differs very little from 656. Hence it follows that in this subsalt the acid is combined with thrice as much base as in the acetate.

The subacetate at a maximum, formed when the preceding subacetate is digested with more oxide of lead, can hardly be obtained in a state of purity in that manner. I prepared it by pouring a great excess of caustic ammonia into the concentrated solution of the preceding subacetate. The mixture is at first transparent; but it soon deposites abundance of subacetate at a maximum. I separated it by a filter, and washed it well with ammonia and water. It was then dried in a vacuum. Heated a little above 212° in the vacuum, it disengaged three per cent. of its weight of water, and acquired a slightly reddish tinge. The 97 parts of residue analyzed by combustion left 91·3 per cent. of oxide of lead. Hence the salt is composed of

Acetic acid	5·70	100
Oxide of lead	91·30	1608
Water	3·00	53

Here the acid is combined with six times as much base as in the neutral acetate, and the water of combination contains half as much oxygen as the oxide of lead.

1·06 parts of effloresced acetate of lead, equivalent to 0·333 of acetic acid, produced by combustion 0·18 of water and 0·574 of carbonic acid. Hence 100 of acetic acid are composed of

Hydrogen	6·35
Carbon	46·83
Oxygen	46·82
	100·00

Now 15·56 × 3 = 46·68. Hence it follows that acetic acid contains three volumes of oxygen. The other numbers correspond with four volumes of carbon and six volumes of hydrogen. Supposing, then, acetic acid composed of 6 H + 4 C + 3 O, its composition will be as follows:—

Hydrogen	6·195
Carbon	46·871
Oxygen	46·934
	100·000

The real capacity of saturation of this acid is 15·63, instead of 15·56, determined by experiment.

There is an approximation between the results of Gay-Lussac and Thenard and mine. According to them, 100 acid are composed of 5·629 hydrogen, 50·222 carbon, and 44·147 oxygen; but when we consider that their experiments were made with acetate of barytes, which, to judge from the quantity of barytes found in it, must have contained a quantity of combined water, the oxygen in which was equal to that in the barytes—when we consider this, it is obvious that they must have obtained much more hydrogen and oxygen than my experiments indicate, instead of a smaller quantity. Probably their balls were so much dried as not only to dissipate all the moisture, but likewise to oxidate a portion of the hydrogen of the acid.

6. *Gallic Acid.*—It is difficult to obtain pure gallic acid. What I employed was obtained by leaving infusion of nutgalls exposed to the influence of the air. The small crystals thus procured were dissolved in water, and crystallized again. I then exposed the

crystals in a small retort to a moderate heat. They at first gave out water, and then sublimed, and left a blackish brown mass, in which it was easy to recognise the presence of tannin; so that these crystals had been composed of gallic acid and tannin. The sublimed acid was colourless, had a bitter taste, and a smell somewhat empyreumatic, owing no doubt to a portion of tannin having been decomposed by the heat.

The sublimed acid does not alter the colour of litmus. It dissolves readily in water, and the solution immediately blackens a solution of iron; but it does not decompose the earthy carbonates. The alkaline carbonates are decomposed by it; and if the mixture contains an excess of alkali, it speedily becomes brown. This decomposition does not appear to take place if the air be excluded.

In consequence of the property which gallic acid has of being decomposed by the influence of the bases with which it is combined, it is difficult to obtain a gallate which is not somewhat altered. To obtain a gallate of lead as pure as possible, I dissolved gallic acid in a very small portion of water, and added carbonate of ammonia in a solid form. I then put the mixture under the receiver of an air-pump, along with sulphuric acid. When the ammonia ceased to effervesce, I pumped out the air. The surplus of ammonia flew off first, and then the water evaporated. The saline mass remaining had a yellowish colour. I dissolved it in warm water, and poured into the solution a boiling solution of nitrate of lead. The whole being put into a phial (which was filled with it), was digested till the gallate of lead assumed the form of grains, which readily separated from the liquid. I decanted off the liquid, and filled the phial with boiling water; and this I repeated till the gallate was sufficiently washed. I then threw it on a filter, and dried it at first by pressure between folds of blotting-paper, and then in a vacuum.

The gallate of lead thus obtained formed a white crystalline powder with a shade of grey. After being exposed to the air for some days, it became brown; but the analytical experiments were made as soon as it became dry.

200 of gallate of lead formed 172·1 of sulphate of lead, equivalent to 127 of oxide of lead. Hence it follows that the salt is composed of

Gallic acid	36·5	100
Oxide of lead	63·5	173·97

The analysis by combustion furnished only 63 per cent. of oxide of lead. The oxygen contained in 173·97 of oxide of lead is 12·44.

Gallate of lead is not easily decomposed by ammonia. The alkali must be concentrated and hot. I obtained by that means a subgallate, which, being dried in the temperature of 230° in a vacuum, where it deposited its water of combination, gave by combustion 84·08 per cent. of oxide of lead. Hence it is composed of

Acid	15·92	100
Base	84·08	528
	100·00	

But 173·97 × 3 = 521·91. Hence in the subgallate the acid is combined with thrice as much base as in the neutral gallate.

One part of gallate of lead, equivalent to 0·365 of acid, produced by combustion 0·1568 water and 0·77 carbonic acid. These quantities indicate 5·05 per cent. hydrogen and 57·37 carbon. Hence the oxygen is 37·68.

0·4 of melted gallic acid produced by combustion 0·17 water, equivalent to 4·994 per cent. of hydrogen, and 0·833 carbonic acid, equivalent to 56·64 carbon. The difference between these two results is owing to the decomposition of the gallic acid, by means of which the quantity of carbon is increased. This acid, then, is composed of

Hydrogen	5·00
Carbon	56·64
Oxygen	38·36
	100·00

But 12·44 × 3 = 37·32, which approaches very nearly to the result of the first experiment; but if these 38·36 are three volumes, we find that the carbon and hydrogen constitute each six volumes; so that gallic acid is composed of 6 H + 6 C + 3 O; which constitutes per cent.

Hydrogen	5·02
Carbon	56·96
Oxygen	38·02
	100·00

From all this it follows, that if we take two volumes of carbon from gallic acid, it becomes acetic acid; if we farther take two volumes of hydrogen, the remainder will be succinic acid.

Thenard and Gay-Lussac, in their Recherches Phys. Chimiques, ii. 321, have established three laws for vegetable combinations:—1. That a substance is acid when its oxygen has to its hydrogen a greater ratio than exists in water between these two bodies. 2. That a substance is resinous, oily, or alcoholic, when its oxygen has to its hydrogen a smaller ratio than in water. 3. That every vegetable substance in which the oxygen bears to the hydrogen the same ratio as in water is analogous to sugar, gum, starch, wood. The analysis of acetic and gallic acids shows us what value ought to be attached to the first and last of these laws. We shall find afterwards, by the analysis of benzoic acid, that the second law is equally erroneous.

Before proceeding further, it will be necessary to make some observations on the substances which have been analyzed. They are all composed of a small number of volumes, compared with those that are to follow; so that the addition of a single volume or atom of one of the elements would produce a great change in the composition. This is the reason why the greater number of vegetable acids do not constitute genera of analogous substances having the same general character, but differing in some points, so that they must be considered as different *species* of the same genus. There is only one acetic acid, one gallic acid, and wherever these bodies occur in the vegetable kingdom they constitute absolutely the same substance.

As the number of volumes increases in the ternary oxides, they acquire the property of producing a certain number of varieties, or of different species of the same genus, which appear to owe their origin to the adhesion of one or more volumes of one of the elements. This addition, however, in consequence of the great number of volumes of the element contained in the substance, only produces an insensible alteration in its composition when we consider 100 parts only.

We have, then, various species of tannin, gum, sugar, &c.; and I have placed the analysis of mucous (*saclactic*) and benzoic acid among the substances which form genera and species, because I think it probable that there are other acids differing from them only as different species. I have found, for example, that *sebacic acid* found in the products of the distillation of fatty substances possesses all the chemical characters of benzoic acid; but that it varies from it a little, owing, I supposed, to the presence of foreign bodies, from which I was unable to free it. It is possible that it may differ from benzoic acid, as the tannin of the oak does from that of catechu, or of the arbutus uva ursi.

I shall not attempt at present to determine the difference of composition of the species. Such experiments appear too delicate to be executed with success before the generic differences are known. To them, therefore, I have directed my first efforts.

7. *Mucous Acid* (*Saclactic Acid**).—Mucate of lead is easily decomposed at a temperature which does not act upon other vegetable salts. It becomes brown; and when afterwards treated with ammonia, this alkali acquires a yellow colour. The same thing happens to the pure acid. Much circumspection is, therefore, requisite, in order to obtain these substances sufficiently dry, without having undergone any decomposition.

A hundred parts of mucate of lead, analyzed by combustion, gave from 51·4 to 51·66 of oxide of lead. Hence 100 acid saturate from 105·75 to 106·87 of oxide of lead; and mucate of lead is composed o

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* The name *mucous acid* is very improper, because it indicates an acid in *ous*, which cannot exist without one in *ic* with the same radicle. I have not, however, ventured to change it, because *mucic* acid might, I thought, appear ridiculous. Its salts, however, I shall call mucates.

I wonder Dr. Berzelius did not think of having recourse to the old name *saclactic acid*, which is very proper, and recalls the history of its original discovery. I have used it ever since I was aware of the absurdity of the term *mucous*.—T.

Acid	48·33	100·00
Oxide of lead	51·66	106·87
	99·99	

According to these experiments, mucous acid ought to saturate a quantity of base containing from 7·56 to 7·64 of oxygen.

Mucate of lead treated by ammonia produces a submucate, but of so glairy a consistence that it was impossible to separate it from the liquid before part of it had absorbed carbonic acid; so that I was obliged to abandon the analysis of it.

0·333 of mucous acid produced by combustion 0·149 of water and 0·393 of carbonic acid. This makes 5·26 per cent. of hydrogen, 31·9 carbon, and 62·84 oxygen.

One part of mucate of lead, equivalent to 0·4833 acid, produced 0·21 water, 0·594 carbonic acid. This gives us per cent.

Hydrogen	5·105
Carbon	33·430
Oxygen	61·465
	100·000

Now 7·66 × 8 = 61·28. Hence it follows that the acid ought to contain eight volumes of oxygen. The other elements approach six volumes of carbon and ten of hydrogen. This would make the composition of the acid

Hydrogen	5·018
Carbon	34·164
Oxygen	60·818
	100·000

But there was some reason for suspecting that the hydrogen was contained in it in nine volumes instead of ten; so much the more as I was not sure that either the mucate or the mucous acid itself were quite dry. I thought it necessary to examine that point in the following manner:—I heated mucate of lead till it lost its white colour by dishydrogenization. The mucate analyzed gave 52·1 per cent. of oxide of lead. Burnt in the apparatus so often mentioned it gave 4·7 per cent. of the weight of the acid of hydrogen, and 35 of carbon. On the supposition that mucous acid is composed of 9 H + 6 C + 8 O, it ought to contain 4·54 per cent. of hydrogen and 34·39 of carbon; but 4·54 is less than 4·7, which the acid (already a little burnt) produced; therefore that acid must contain more than nine volumes of hydrogen; therefore the composition of the acid must be 10 H + 6 C + 8 O, and the excess of hydrogen and oxygen found in the analysis must be owing to humidity.

Gay-Lussac and Thenard found mucous acid composed of 3·62 per cent. hydrogen, 33·69 carbon, and 62·69 oxygen. This (the hydrogen excepted) does not differ much from my result.

When we compare the analyses of the tartaric and mucous acids, we find between them so little difference, that we should guess their composition to be very analogous. Yet we see by the analysis of mucate of lead, and by the capacity of saturation of mucous acid, that the arrangement of their atoms is very different.

8. *Benzoic Acid.*—The benzoate of lead is a light crystalline powder, slightly soluble in water. When heated in a temperature somewhat higher than that of boiling water, it melts, and gives out 3·85 per cent. of water. If the heat is too long continued, or raised a little higher, a portion of the acid begins also to evaporate. We must, therefore, employ a retort, to determine the water of combination of this salt; for if a little acid should sublime, it will be deposited on the upper part of the retort.

Three parts of benzoate of lead, cautiously melted, and treated with sulphuric acid diluted with alcohol, produced 1·933 of sulphate of lead, equivalent to 1·4505 of oxide of lead. Hence benzoate of lead is composed of

Benzoic acid	51·65	49·66	100
Oxide of lead	48·35	46·49	93·61
Water		3·85	
	100·00	100·00	

Now these 46·49 of oxide of lead contain 3·32 of oxygen, and the 3·85 of water contain 3·39 oxygen; so that the water and oxide of lead contain equal quantities of oxygen.

Fused benzoate analyzed by combustion gave 49·125 per cent. of oxide of lead, which makes 94·05 for 100 of acid. Hence this acid neutralizes a quantity of base containing from 6·69 to 6·72 of oxygen.

Neutral benzoate treated with concentrated ammonia gives a subbenzoate, which being analyzed by combustion gives 74 per cent. of oxide of lead. Though long exposed to a high temperature, it gives no trace of water, not even when its acid begins to be disengaged. Hence it is composed of

Benzoic acid	26	100
Oxide of lead	74	284·6
	100	

But 94 × 3 = 282. Therefore benzoic acid ought to contain three volumes of oxygen.

The analysis of benzoic acid cost me much more trouble than any of the others owing to a circumstance which I was long in discovering. Its weak affinity and great volatility occasions the evaporation of a portion of it during the combustion. The portion evaporated is not decomposed, and, being mixed with the gas, it is not deposited in the water, but is carried probably in the form of gas into the tubes and pneumatic trough. In consequence of this circumstance, the analysis of this acid always gave a loss too great to be ascribed to oxygen. I thought at first that my analyses of the benzoates were inaccurate; but having repeated them with the same result, it became obvious that the cause of the anomaly must be something different. After having made analytical experiments on benzoic acid in a free state, and in that of benzoate and subbenzoate, I found that the loss diminished in proportion as the acid was more strongly retained by the base; so that the subbenzoates gave a less loss than the other salts. This induced me to examine whether there was really an evaporation of undecomposed acid during the experiments. I observed that the vessels through which the gas had passed, and in which it remained, had diminished in transparency, though not very remarkably. As these experiments were mostly made in the very severe winter of 1813-14, it happened sometimes that the temperature of the mercury in the trough was at 3° or at zero. On this account a greater quantity of the acid had been deposited in the part of the tube which entered the mercury, where I found sufficient to be collected and examined. It possessed the characters of benzoic acid; but its odour was a little empyreumatic.

The analysis of a quantity of subbenzoate of lead, containing 0·317 of acid, gave 0·1414 water and 0·8645 carbonic acid. Hence the acid is composed of

Hydrogen	5·16
Carbon	74·41
Oxygen	20·43
	100·00

The most simple ratio in which these bodies can be combined is 3 O + 12 H + 15 C, or (which comes to the same thing) O + 3 H + 5 C. By turning these volumes into numbers we have the composition of the acid per cent. as follows :—

Hydrogen	5·27
Carbon	74·71
Oxygen	20·02
	100·00

Now 6·69 × 3 = 20 07. Hence it follows that the analysis of the subbenzoate has given an exact result; while neither the benzoate nor fused benzoic acid ever gave more than 4·8 hydrogen and 70 carbon.

9. *Tannin from Nutgalls.*—I prepared a cold infusion of nutgalls in water, which I mixed with ammonia till it ceased to act as an acid. I then added a little of the infusion not mixed with

ammonia till I restored the property of reddening litmus paper, that there might be no excess of ammonia. The tannate of ammonia thus formed mixed with a solution of muriate of barytes lets fall abundance of tannate of barytes. I separated it by a filter, and washed it with care. From the experiments of Sir H. Davy, we know that the gallate of barytes is soluble in water; so that in this experiment it is only the tannate which is precipitated. The tannate of barytes mixed with diluted sulphuric acid furnished a super-tannate of barytes soluble in hot water. To this solution I added diluted sulphuric acid till almost the whole was decomposed. I filtered the liquid thus obtained, which had a very astringent taste, and strongly reddened litmus paper. This last property was not owing to the presence of sulphuric acid, for the liquid still retained tannate of barytes in solution. I now mixed it with caustic ammonia till the tannate of barytes began to precipitate. This solution of tannate of ammonia, with a small excess of tannin, was mixed with neutral nitrate of lead as long as any precipitate fell. The tannate of lead thus obtained had a yellowish colour, and became somewhat brown while washed. On examining the combination of tannin with oxide of lead, I found that the oxide has a strong propensity to combine with an excess of tannin, and that the precipitate obtained was in reality a mixture of tannate and super-tannate of lead. I found that the super-tannate might be rendered neuter by boiling it in water as long as that liquid separates tannin. The water dissolves no part of the oxide; and the tannate which remains undissolved is neutral. If the precipitation of the tannate takes place at 212° there is very little super-tannate formed.

Neutral tannate thus prepared was dried in a vacuum, not to be exposed to the influence of the oxygen of the air, by which it is a little altered.

Tannate of lead obtained at different times, though by the same means, was analyzed by combustion. It gave 34·12, 34·21, 34·56 per cent. of oxide of lead, probably as the excess of tannin was more or less completely removed. Hence this tannate is composed of

Tannin	65·79	100
Oxide of lead	34·21	52
	100·00	

Now 52 oxide of lead contain 3·718 of oxygen.

When I attempted to obtain a subtannate of lead by means of the action of ammonia on the neutral tannate, I obtained a tannate of a browner colour, and very mucous; but which, after being washed and dried in a vacuum, gave 34·6 per cent. of oxide of lead. Hence it would appear that ammonia does not decompose the neutral tannate; just as happens when we add an excess of alkali to the tannate of the same alkali.

There is, however, a subtannate which I obtained by precipitating tannate of ammonia with a boiling solution of subnitrate of lead.

This compound is white; and, when dried, it becomes greenish; but I have never obtained it of the same degree of saturation when prepared by different processes. The tannin in it was always combined with more than $1\frac{1}{2}$ times as much base as in the neutral tannate; though it never contained so much as twice that quantity.

I analyzed tannin by employing for the combustion both supertannate and tannate as neutral as possible, and the analyses furnished the same result: 0·4 of tannin produced 0·1425 water and 0·7625 carbonic acid. Hence tannin of nutgalls is composed of

Hydrogen	4·186
Carbon	51·160
Oxygen	44·654
	100·000

But we have seen that 100 of tannin combine with 52 oxide of lead, the oxygen in which is 3·718. Now 3·718 × 12 = 44·616. Hence we may conclude that tannin contains 12 volumes of oxygen. But there is no doubt that there must be an analogy between gallic acid and tannin, and this analogy can scarcely be any thing else than the same compound radicle combined with different volumes of oxygen. Supposing, then, that tannin, like gallic acid, contains equal volumes of carbon and hydrogen, and that it is 12 O + 18 C + 18 H, or (which comes to the same thing) 4 O + 6 C + 6 H, its composition ought to be per cent.

Hydrogen	4·45
Carbon	50·55
Oxygen	45·00
	100·00

Here we find a little more hydrogen and a little less carbon than the analysis indicates. This is a necessary consequence of the dishydrogenation which tannin undergoes by exposure to the air, in consequence of which the colour of its combinations becomes darker and darker; so that we never can procure this substance in a perfect state, excepting in fresh nutgalls in which the tannin is not yet coloured.

(To be continued.)

ARTICLE IV.

Observations on some Points connected with the Atomic Theory. By Thomas Thomson, M.D. F.R.S.

PROFESSOR BERZELIUS, having in his important dissertations on this subject published in the second, third, fourth, and fifth volumes of the *Annals of Philosophy*, pointed out some difficulties which present themselves when we apply the atomic theory to the salts, and having in a recent paper refused to admit Mr. Dalton's attempts to remove these difficulties as valid—perhaps the reader will not be displeased if I state here, in as few words as possible, how I have been in the habit of viewing these difficulties when they happened to present themselves during my examination of the different genera of salts. When a difficulty occurs in any branch of chemical investigation, the greater number of persons there are who attempt to explain it, so much the sooner, in all probability, will it be removed. Even should my explanations not prove perfectly satisfactory, they may serve to convey a lucky thought to some other person, who may be more fortunate in his endeavours.

1. It appears from the tables of the sulphates, carbonates, and nitrates, published in the second and third volumes of the *Annals of Philosophy*, that the yellow oxide of lead combines always with two integrant particles of acid when it constitutes neutral sulphate, carbonate, and nitrate of lead. The same law holds in the phosphate, borate, oxalate, and all the other neutral salts of lead.

Nitrate of lead is composed of	$2n + 1l$	41·580
Sulphate of potash	$1s + 1p$	11·000
		52·580

Now if we mix 41·58 grains of nitrate of lead with 11 grains of sulphate of potash a double decomposition will take place, and two neutral salts will be produced, namely, nitrate of potash and sulphate of lead, composed as follows:—

Nitrate of potash	$1n + 1p$	12·803
Sulphate of lead	$2s + 1l$	37·974
		50·777

Now it is obvious to the eye that if the two salts be composed as we have supposed, such a double decomposition is impossible. The first two salts contain two integrant particles of nitric acid; the last two, only one integrant particle; while, on the contrary, there is only one integrant particle of sulphuric acid in the first two salts, but two in the last two. The weight of the first two salts is 52·58 grains; that of the last two, 50·777 grains. Thus about two grains of weight are lost by the decomposition; while a particle of nitric acid must be transmuted into a particle of sulphuric acid. The same absurdities and contradictions will be found to take place whenever we attempt to reduce any double decomposition, by means of a salt of lead, to calculation.

It is very obvious, from the appearance of these contradictions and absurdities, that there must exist some error in our tables; that the salts of lead cannot be constituted as we have supposed them. Now a very slight alteration will remove all the anomalies, and render the composition of the salts of lead quite conformable to experiment. Reduce the weight of an integrant particle of yellow oxide of lead to one-half the weight which I have given it in my original table, published in the second volume of the *Annals of Philosophy*. Let the weight be 13·987 instead of 27·974. In that case all the salts of lead will be composed of one integrant particle of acid and one integrant particle of oxide of lead; we have

Nitrate of lead composed of	$1\,n + 1\,l$	20·790
Sulphate of potash	$1\,s + 1\,p$	11·000
		31·790

Forming,

Nitrate of potash	$1\,n + 1\,p$	12·803
Sulphate of lead	$1\,s + 1\,l$	18·987
		31·790

Here the double decomposition is obviously possible. The weight of the first two salts is just the same as that of the last two, and the number of integrant particles is the same in both.

It seems to me to be absolutely necessary to remove this anomaly from the salts of lead; nor do I see any other method of doing so except the one I have just now proposed; but if we reduce the weight of an integrant particle of yellow oxide of lead to one-half, it is obvious that it must no longer be considered as a compound of 1 atom lead + 2 atoms oxygen, but of 1 atom lead + 1 atom oxygen. It will be a protoxide instead of a deutoxide. If so, provided it be true that the brown oxide contains just twice as much oxygen as the yellow oxide, numbers adopted on the authority of Berzelius, the brown oxide must be a deutoxide of lead composed of 1 atom lead + 2 atoms oxygen. The red oxide, which is intermediate, must be considered as a compound of one integrant particle of yellow oxide and one integrant particle of brown oxide. This notion was first suggested by Proust, and appears to have been adopted by Mr. Dalton. I own that in consequence of the anomaly which I have just pointed out I have been for some time inclined to the same opinion; but a desire previously to examine the properties of the red oxide of lead under this point of view has hitherto prevented me from making any alteration in the weight of an atom of lead. Berzelius refuses to adopt this opinion. There is, no doubt, another method of getting rid of the anomaly which has been pointed out above without having recourse to it; namely, to double the weight of an atom of all the other metals; but that method would be attended with much greater inconveniences, and could not therefore be adopted with propriety. I do not see any alternative, therefore, in the present state of our knowledge, but that of adopting the opinion of Proust respecting the nature of *red lead*, and considering yellow oxide of lead as a protoxide.

If any person has taken the trouble to study the tables of the salts which I have published, he will have been struck with some other anomalies of a similar nature to those belonging to the salts of lead; but I forbear touching upon them at present, till we are in possession of more perfect analyses than have been hitherto published.

2. Certain salts have been analyzed, the composition of which, when stated in symbols, presents us with the anomaly of $1\frac{1}{2}$ atoms of one substance combined with one atom of another. Thus subsulphate of iron is composed thus, $1\,(S\,O^3) + 1\frac{1}{2}\,(F\,O^2)$; or it contains one integrant particle of sulphuric acid and $1\frac{1}{2}$ integrant particle of black oxide of iron. As such a composition is obviously impossible, it is clear that in such cases a certain number of integrant particles of sulphuric acid must be combined with a certain number of integrant particles of oxide of iron. If we multiply by two, the anomaly will disappear. We shall then have $2\,(S\,O^3) + 3\,(F\,O^2)$; that is to say, the salt is a compound of two integrant particles of sulphuric acid and three integrant particles of black oxide of iron. Such combinations do not present any thing incompatible with the atomic theory, which admits of such combinations; but they constitute exceptions to one of the general laws which Berzelius has deduced from his numerous analyses. The law is, that in all inorganic compounds one of the constituents always enters in the state of a single atom. In the subsulphate of iron there is no single atom, either simple or compound. It consists of 12 atoms of oxygen, three of iron, and two of sulphur; or of two integrant particles of sulphuric acid and three integrant particles of black oxide of iron. The composition of the subsulphate of copper may be explained in the same way. Its symbol is $1\,(S\,O^3) + 1\frac{1}{2}\,(C\,O^2)$, or one integrant particle of sulphuric acid combined with $1\frac{1}{2}$ integrant particle of black oxide of copper. If we multiply by two we obtain $2\,(S\,O^3) + 3\,(C\,O^2)$, which I conceive to be the real constitution of the salt; namely, a compound of two integrant particles of sulphuric acid with three integrant particles of black oxide of copper. The subarseniate of lead admits of the same explanation. Its symbol is $1\,(As\,O^3) + 1\frac{1}{2}\,(P\,O^2)$, or one integrant particle of arsenic acid combined with $1\frac{1}{2}$ integrant particle of yellow oxide of lead. When we multiply by two we obtain $2\,(As\,O^3) + 3\,(P\,O^2)$; that is, two integrant particles of arsenic acid united with three integrant particles of yellow oxide of lead.

Various other similar examples might be adduced; but they are all explicable in the same way. They appear to me to constitute so many exceptions to Berzelius's law above-mentioned, and to show that it does not hold so universally as he had supposed.

3. Professor Berzelius has just favoured the chemical world with an analysis of a considerable number of the vegetable acids, and some other vegetable substances. That these experiments have been conducted with the greatest care is evident from the details into which Berzelius has entered; nor have I the least doubt, from the uncommon precision which characterizes whatever is done by this

very skilful chemist, that his results furnish very near approximations to the truth; but from the extreme difficulty attending such analyses, I do not conceive that perfect precision can be attained at first; but I think that the atomic theory furnishes us with such additional checks that, by a judicious application of them to the constitution of these acids as determined by Berzelius, we may obtain results approaching exceedingly near the truth. I shall at present apply that theory to the investigation of the composition of oxalic acid.

According to Berzelius, that acid is composed as follows:—

Oxygen	66·534	6 atoms
Carbon	33·222	4
Hydrogen	0·244	1
	100·000	11

But as I consider the weight of an atom of hydrogen as twice as great as Berzelius makes it, we must, in order to represent the composition of this acid according to my numbers, double the number of atoms of oxygen and carbon which it contains. It will then be composed of

Oxygen	12 atoms
Carbon	8
Hydrogen	1
	21

So that it is a very complicated body. The weight of an integrant particle of it must be 18·140. We might, indeed, reduce the number of atoms in this acid to 11 by doubling the quantity of hydrogen, obtained by Berzelius; for the difference seems to be within the limits of the unavoidable errors to which such experiments are liable. But Berzelius does not think that he could have committed any such error. Let us, therefore, have recourse to another method of determining the weight of an integrant particle of oxalic acid.

From the analysis of oxalate of lead made with great care by Berzelius it appears to be a compound of

Oxalic acid	100
Yellow oxide of lead	307·5

Now there can be no doubt, from a comparison of all the genera of salts hitherto examined, that oxalate of lead is a compound of two integrant particles of oxalic acid and one integrant particle of yellow oxide of lead; so that to find the weight of an integrant particle of oxalic acid we have this proportion:—$307{\cdot}5 : \frac{100}{2} :: 27{\cdot}974 : 4{\cdot}548$ = an integrant particle of oxalic acid. Now this is just the fourth part of the weight of that acid resulting from the analysis of Berzelius; yet it is the weight of an integrant particle of oxalic acid, as any one may satisfy himself by examining the composition of the oxalates, a table of which I subjoin for the satisfaction of the reader:—

	Number of atoms.	Weight of an integrant particle.
Oxalate of potash	1 *ox* + 1 *p*	10·634
Binoxalate of potash	2 *ox* + 1 *p*	15·268
Quadroxalate of potash	4 *ox* + 1 *p*	24·536
Oxalate of soda	2 *ox* + 1 *s*	17·150
Super-oxalate of soda	3 *ox* + 1 *s*	21·784
Oxalate of ammonia	1 *ox* + 1 *a*	6·783
Binoxalate of ammonia	2 *ox* + 1 *a*	11·417
Oxalate of magnesia	1 *ox* + 1 *m*	7·211
——— lime	1 *ox* + 1 *l*	8·254
Binoxalate of lime	2 *ox* + 1 *l*	12·888
Oxalate of barytes	1 *ox* + 1 *b*	14·365
——— strontian	1 *ox* + 1 *st*	11·534
——— alumina	1 *ox* + 1 *al*	6·770
——— yttria	1 *ox* + 1 *y*	13·034
——— glucina	1 *ox* + 1 *gl*	14·467
——— zirconia	1 *ox* + 1 *z*	10·290
——— copper	2 *ox* + 1 *c*	19·268
——— potash-and-copper	2 *ox* + 1 *p* + 1 *c*	29·902
——— soda-and-copper	3 *ox* + 1 *s* + 1 *c*	32·410
——— ammonia-and-copper	2 *ox* + 1 *a* + 1 *c*	26·051
——— iron	2 *ox* + 1 *i*	18·383
Peroxalate of iron	3 *ox* + 1 *i*	23·017
Oxalate of nickel	2 *ox* + 1 *n*	18·573
——— cobalt	2 *ox* + 1 *c*	18·594
——— lead	2 *ox* + 1 *l*	37·242
——— zinc	1 *ox* + 1 *z*	9·661
——— mercury	1 *ox* + 1 *m*	30·634
——— silver	1 *ox* + 1 *s*	19·348
——— bismuth	1 *ox* + 1 *b*	14·628
——— manganese	2 *ox* + 1 *m*	17·101
——— uranium	1 *ox* + 1 *u*	19·634
——— cerium	2 *ox* + 1 *c*	23·115
——— platinum	1 *ox* + 1 *p*	17·795

All these salts corroborate the weight of an integrant particle of oxalic acid as deduced from the analysis of oxalate of lead. Here, then, we have two experiments of Berzelius, which are inconsistent with each other; namely, the analysis of oxalic acid and the analysis of oxalate of lead. Both were made with the greatest care; but as they are inconsistent with each other, they cannot both be correct, and there can be no hesitation about which of them ought to be followed. The analysis of oxalate of lead is much simpler, not liable to the same uncertainties, and susceptible of greater exactness than the analysis of oxalic acid. We ought, therefore, to be guided by it; especially as it is corroborated by various other very exact analyses, as those of oxalate of potash, oxalate of ammonia, and oxalate of strontian. But if we adopt this determination, and compare it with the analysis of oxalic acid, we shall find that this acid must be composed of six atoms; namely, three atoms oxygen, two carbon, and one hydrogen; and its composition will be

				Weight.
Oxygen	64·739	or 3 atoms	=	3·000
Carbon	32·413	2	=	1·502
Hydrogen	2·848	1	=	0·132
	100·000			4·634

Now this is the composition of the acid which I deduced some time ago by comparing my own analysis of oxalic acid with the composition of oxalate of lead as determined by Berzelius. I obtained for the composition of oxalic acid

Oxygen	64
Carbon	32
Hydrogen	4
	100

My experiment was conducted with great care, and I still consider my result as nearer the truth than either that obtained by Gay-Lussac and Thenard or by Berzelius. My excess of hydrogen amounts to about one per cent., and was probably owing to the salt which I employed in the analysis not being quite free from water. Mr. Dalton has adopted the same constitution of oxalic acid with the above, and probably he has been led to it by the same mode of reasoning from which I deduced it.

I shall take another opportunity of examining the other acids analyzed by Berzelius, by applying to them the test of the atomic theory. What I have here said is sufficient to show us that the most cautious and elaborate experiments are not sufficient of themselves to make us acquainted with the composition of these intricate bodies; though such experiments afford us considerable assistance, and, when compared with the structure of the salts as explained by the atomic theory, will generally be sufficient to give us all the information respecting the constitution of these acids which we can expect to obtain.

Article V.

Observations on the Uses of the Dorsal Vessel, or on the Influence which the Heart exercises in the Organization of articulated Animals, and on the Changes which that Organization experiences when the Heart or the Organ of Circulation ceases to exist. By M. Marcel de Serres.

(*Continued from* Vol. iv. p. 355.)

I began the examination of the dorsal vessel with those species in which we see it beating externally. Among those I chose the larvæ of the coleopteræ and lepidopteræ. The larva of the geotrupa nasicornis, being very common, seemed proper for my object. The dorsal vessel of this species is elongated and cylindrical. When separated from the muscles and fatty membranes which surround it, we see that its diameter is the same in almost the whole of its length, being only a little contracted at the two extremities. Having fully ascertained this disposition, I endeavoured to ascertain if there were any ramifications. For this purpose I examined it with the greatest attention, and with the best glasses. The contractions were always confined to the dorsal vessel, and never extended beyond the canal which runs along the back. I then placed this vessel under the lens of my microscope, and could not perceive any ramifications, not even in the membranes which surround it. In vain I endeavoured to find some trace of them in the membrane of the intestinal tube, the fibres of the muscles, especially in those of the rings of the abdomen and mandibles, which ought to have presented them, if any had existed, in consequence of the energy of their contraction, and the need which these organs have of vessels.

I then examined the dorsal vessel of the geotrupa nasicornis et punctata; but all my attention was unable to discover the least ramification. I subjected to the same examination a very considerable number of coleopteræ, the largest that I could procure, as the *ateuchus semi-punctatus, cetonia aurata et fastuosa, scarites gigas, cerambyx heros, blaps gigas,* and *mortisaga.* In all of them I observed the dorsal vessel without any ramifications. These dissections, however, convinced me that, without a certain attention, ramifications of that vessel may be supposed to exist, on account of the colour and disposition of the hepatic vessels, which, being long and almost capillary, spread over every part of the body, and are often found fixed to it after the intestinal tube has been removed. To determine with certainty this disposition, we must allow the intestinal tube to remain, and dissect in water. This liquid lifts up the hepatic vessels; so that it becomes easy to follow them to their insertion.

Though I could not perceive any ramifications in these species, I was not entitled to conclude that they did not exist in insects. I therefore made new dissections. The *locusta gygantea* being the largest insect in Europe, I made it the subject of my new observations; but I was not more fortunate in that species than I had been in the preceding. The same thing happened with the *locusta brevipennis, verrucivora, grisea, gryllus lineola,* and *migratorius.* Though all these species are of a considerable size, I tried new dissections on the *blatta occidentalis, acheta campestris, empusa pauperata,* and *mantis religiosa.* In all these I found the dorsal vessel without any ramification; and what assured me of this was, that after removing it with the greatest care, I could perceive no trace of rupture, which would have been the case if it had been torn from the ramifications of that vessel. No liquid was ever seen to run out; but this, as we shall show hereafter, might depend upon a variety of circumstances.

But it was necessary to verify the observations of Comparetti. For that purpose I examined the dorsal vessel in a great number of *butterflies, tenebrios,* and *crickets.* All these dissections confirmed the results that I had already obtained. The same was the case when I studied the organization of the *sphinx, noctua,* and *telligonia,* all of them insects of a large size. It remained to be seen whether the dorsal vessel of flies and of syrphus presented any ramifications, as Comparetti has described such with considerable detail. I saw in general in the dipteræ and hymenopteræ the dorsal vessel exhibiting numerous contractions; but I was never able to perceive either vessels or pulsations along the side of the dorsal vessel. Yet I studied this vessel in the *scolia flavifrons, apis violacea,* and *syrphus bifasciatus* of Panzer, the largest species to be found in the south of France. I then took the *apis mellifica,* which Comparetti mentions expressly. Though the research in this case was more difficult, on account of the smallness of the species, I did not perceive the cylindrical vessels, of which that anatomist speaks, and which according to him proceed from the extremity of the dorsal vessel, one going to the upper part of the body, the other to the lower. Though the disposition here mentioned by Comparetti indicates the regular course of a vessel, I still believe he was deceived by the appearance of the hepatic vessels.

I terminated these first researches by the examination of the larvæ of the lepidopteræ. I chiefly dissected those of the sphinx, of the tithymale, and of the bombyx of the mulberry. All these larvæ appeared to me to have a dorsal vessel without any kind of ramification. I sought likewise to discover some traces of vascular vessels in the cellular or muscular coats of the intestines—coats which certainly would have received them if any had existed, for it is very well known that the digestive apparatus is very distinct in larvæ. The muscles of the mandibles, examined with the same design, appeared equally destitute of all vestiges of vessels, with whatever care I examined them.

These facts prove that the dorsal vessel in insects is a canal almost cylindrical, only a little tapering at its two extremities. Its contractions are so distinct as to be perceptible on the outside of the body when the skin is not thick, and has a certain degree of transparence. The contractions of this vessel are irregular, and scarcely ever isochronous. This irregularity shows that they are not produced by a liquid in circulation. We shall find hereafter that they do not depend upon that liquid, and that they appear even independent of the organization of the dorsal vessel. These contractions are not equally strong, nor equally numerous, in all the species. Though it is difficult to determine any thing precise on this head, yet they seem more distinct in the larvæ than in the perfect insect. This difference is least distinct in the voracious larvæ, as those of the geotrupa nasicornis, and certain species of sphinx and bombyx.

As to the liquid contained in the dorsal vessel, its colour has always a relation to that of the adipose substance which surrounds this same vessel; consequently it is not uniform in the different species. We observe in fact that the fat, which surrounds the dorsal vessel, has always a colour analogous to that of the liquid contained in the dorsal vessel. This constant similarity of colour may lead to the suspicion that the dorsal vessel is destined for the secretion of that matter so necessary in animals, whose parts increase rapidly when they are transformed into new organs different from those which existed before.

The humour of the dorsal vessel, then, exhibits various shades of colour. It is deep brown in most caleopteræ, greenish in certain orthopteræ, yellow in the silk-worm, orange in the caterpillar of the willow, transparent in the larvæ of the great butterfly (*paon*), and of a very light colour in most of the lepidopteræ. When examined before the microscope, this humour appears composed of a great number of globules, the transparency of which depends upon the colour of the humour itself. This organization announces an analogy of this humour with fat. Like it, we see it composed of small grains, which before the microscope appear each to consist of other smaller grains. These grains are merely little globules of fat, which swim, or are contained in small spherical and membranous sacks. The fat is contained in membranous sacks; for of itself it is fluid, and runs out easily when the sacks are pierced with the point of a fine needle. When thus extracted, it renders water muddy, and divides into small masses. The humor of the dorsal vessel put into water easily mixes with that liquid. A drop of it spread upon a piece of porcelain hardens by evaporation, and then resembles gum.

The coats of the dorsal vessel are in general very thin. It would be difficult to say to what class they belong. As far as I can judge, the external membrane is cellular, and the internal muscular. The dorsal vessel is kept in its position by numerous tracheæ, several of which lose themselves in it. It is probable that these tracheæ, by their crossing each other in every direction, form the outer coat of the vessel. What proves this is that, in certain species, as the larvæ of the *bombyx pavonia major*, we see the dorsal vessel as if formed of longitudinal white streaks. These streaks are merely the tracheæ extending along the dorsal vessel, and forming on it an inextricable network. It is easy to satisfy oneself that these vessels are tracheæ, by plunging them into a coloured liquid. The whole becomes coloured except the tracheæ, which we see always distinctly, and the principal direction of which is longitudinal. This fact, easily verified, proves, I conceive, that the first membrane of the dorsal vessel is cellular; since in certain species we see distinctly the white fibrillæ of the tracheæ which compose it lose themselves in it. At least we observe that the tracheæ have a great influence on the contractions of the dorsal vessel—contractions always strongest in the points where these exist in great numbers. As it is always the inferior portion of the body that receives the greatest number of tracheæ, so it is the extremity of the dorsal vessel in which the contractions are most frequent and most sensible.* We may say, then, in general, that the contractions of the dorsal vessel are always stronger in the abdomen than elsewhere; and that on account of the great number of tracheæ found there, and of the direct communication of these tracheæ by means of stigmata with the air. In the larvæ which receive air by the anus, as the libellulas, this fact is still more evident.

If the tracheæ, or, to speak more correctly, the air which they introduce, have a direct influence on the contraction of the dorsal vessel, this ought to be the better perceived if we examine the vessel in the different transformations which the insects undergo. It is known that the same organs, or the same system of organs, undergo great changes in insects, according to the state in which they are. When in the larva state, the digestive system is most preponderant, and the tracheæ passing into the intestinal tube are so numerous that they form very numerous bundles round the intestines, and very multiplied nets. This disposition is particularly evident in the caterpillars. The caterpillars which inclose themselves in cocoons have likewise this peculiarity, that the silky vessels become very large at the time that they are going to change into crysalids. These vessels, then, fill all the cavity of the abdomen, and the intestinal tube diminishes so much that it appears to contract upon itself, and to be reduced almost to nothing. This is particularly the case with the worms that spin a great deal, as the silk-worm, the *bombyx pavonia major*, *media*, *trifolia*, *quercus*, &c. In the perfect insects the organs of generation, and those of motion, assume the greatest preponderance; the tracheæ become likewise very numerous in these parts. Here is a very sensible change produced in the organs of insects, a change which is the consequence of their successive transformations. But does the same thing take place with the dorsal vessel? To determine the point, I compared the dorsal vessel of larvas with those of perfect insects.

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* Lyonnet has made that remark in his excellent Anatomie de la Chenille du Saule.

I found that in general the diameter of the dorsal vessel was much more equal in larvæ than in perfect insects. I first thought that the form of the body which in the larvæ exhibits few contractions, but is almost always cylindrical, might be the cause of this equality. But though this cause may have some influence, I do not think it the principal one. It seems to be chiefly owing to the much greater equality with which fat is distributed over the bodies of larvæ than of perfect insects. In those insects which have no fat except in the abdomen, the dorsal vessel is scarce sensible in the thorax, or at least its contractions. But in all, the contractions are always greater in the abdomen, in consequence of the tracheæ which pass into it, as has been already said. But as an organ never becomes larger without the object which it has in view feeling the consequence of it, the fat is much more abundant in the abdomen than any where else. Though this is less evident in the perfect insects than in the larvæ, for reasons already explained, the abundance of adipose matter in the abdomen is still very sensible in them. We shall see in a future part of this memoir how far these facts will enable us to determine the functions of the dorsal vessel.

The dorsal vessel of insects receives nerves, and these in considerable number. Lyonnet * mentions nine pair in describing the dorsal vessel of the caterpillar of the willow. We cannot fix the number so precisely, especially when we take a general view of this vessel; but it appears to us that it always receives more in the larvæ than in the perfect insects. Nerves seem even to enter and lose themselves in the membranes of this vessel; but we have not been able to verify the observation of Lyonnet, who affirms that several nerves issue from this organ. This skilful observer perceiving the dorsal vessel to receive a great number of nerves, thought that it furnished to the nerves the substance necessary for them. It would be very difficult to prove the truth of such an assertion, or to demonstrate its falsehood. We think it sufficient to mention it, without admitting it; for to judge from analogy, one would not be disposed to believe it.

Before speaking of the muscles of the cellular membrane which keep the dorsal vessel in its position, we shall say a few words on the contractions and dilatations of that organ. This vessel, we have said, exhibits stronger contractions and dilatations in the larvæ than in perfect insects. This is true in general with this restriction, that if these contractions are more distinct, they are on the contrary less frequent. Thus I reckoned only 36 pulsations in the minute in the dorsal vessel of the larva of the large butterfly (*paon*), while I observed more than 82 in the locusta, and more than 140 in the *bremus terrestris*; but in the larvæ the contractions were so strong that the two sides of the membrane of the dorsal vessel touched one another when the contraction was at its maximum. In the grasshopper, on the contrary, the contractions carried these membranes scarcely one-fourth of the diameter of the dorsal vessel. The motion perceived in the bremus terrestris was rather a continual pulsation than a contraction and dilatation of the membranes of the dorsal vessel. These pulsations, which I reckoned at 140 in a minute, but which in reality are much more frequent, proceed from the bottom to the top, and are very irregular. The contractions and dilatations of the dorsal vessel are so strong in the caterpillars that they shake all their peritoneal membrane and their adipose tissue; which never takes place in the perfect insects. It is obvious that when the dorsal vessel contracts, its diameter must diminish, while it increases in the dilatations.

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* See the Traité Anatomique de la Chenille du Saule, chap. xi. p. 412, pl. xii. fig. 1.

The dorsal vessel is maintained in its position by muscular fibres, which in general are disposed in triangles, and which increase in breadth from the superior part of the body to its inferior extremity. The muscular fibres have been called wings by Lyonnet, because they form a kind of appendix, which puts one in mind of that organ. These appendices, formed of muscular strings, contain a great number of very small fatty molecules, and some nerves; but these muscular strings, when carefully examined before the microscope, appear composed of very fine and close transverse fibres. Besides these fibres, we see that the fatty matter is contained in a very extensible cellular substance. It is in the numerous cells of this substance that we find a very abundant adipose matter, the colour of which, always analogous to that of the dorsal vessel, is distinguished from the white and more animalized fat contained in the peritoneal membrane. We may conclude that this fat is more animalized than that which touches the dorsal vessel, because its properties are nearly similar to that of animals with vertebræ.

The colour of this fatty substance is almost always the same. It is generally white, and varies only in its shade from snow-white to yellowish-white. Its odour is faint, seldom disagreeable, and often almost nothing. It has no action on vegetable colours. It is very dense; and when disengaged from the membrane that contains it, it falls to the bottom of water. When exposed to the air, it becomes yellow and rancid, yielding a portion of its carbon and hydrogen to the oxygen of the air. I cannot say whether, as is the case in animals with vertebræ, the fat of frugivorous insects is firmer than that of carnivorous insects. All the acids act on the fat of insects with more or less energy. Sulphuric acid chars it very readily, as I have observed in the fat of the *bombyx pavonia major*. The fat of the larva of this species dried in the air, and of consequence become rancid, and then acted upon by sulphuric acid, gives that acid a strong yellow colour, and does not char very quickly. When the fat of insects is treated with nitric acid, a little nitrous gas is exhaled. The fat becomes yellow, coagulates rapidly, combines with a certain quantity of oxygen, and formsa kind of oxygenized pommade. The fat of the *gryllo-talp* treated with muriatic acid coagulates very quickly, but becomes less yellow, the muriatic acid giving it less oxygen.

From all that has been said, it is evident that it is exceedingly difficult, not to say impossible, by direct observations to know the ramifications of the dorsal vessel; but it would have been improper to have admitted proofs purely negative, without having attempted direct observations in the first place. The first means that presented itself was injections. To them, therefore, I had recourse in the first place.

The membranes of the dorsal vessel being very thin, oppose a great obstacle to the success of mercurial injections. Accordingly, the dorsal vessel almost always bursts before the injection has reached its extremity; so that we cannot depend upon the results of such injections. Yet I ought to observe that, in those parts of the dorsal vessel in which the injection has succeeded, I have never observed the mercury make its way into a single fibrilla, or indicate the smallest ramification. This method appearing to me insufficient, I soon abandoned it, and tried various injections with liquids coloured with cochineal or indigo, according to the colour of the dorsal vessel. These injections, forced in by a very fine syringe, succeeded perfectly, especially in the larvæ of the great butterfly (*paon*), and of the sphinx of the lithymale, and likewise in the mole cricket. In all of them, the injection having penetrated to the extremity of the dorsal vessel, this vessel always appeared to me a simple cylindrical canal without any ramification. I repeated these injections in a great number of individuals of each class, and I always had the same result.

Though these coloured liquids did not show any ramifications of the dorsal vessel, it was possible that these ramifications might become sensible by making this vessel absorb these liquids. For this purpose I put living insects into liquids strongly coloured, and allowed the absorption to take place through the pores of the organs. I had the greater hopes of success, because the conformation of the tracheæ gave me hopes that these vessels would absorb very little o the coloured liquids. In all the insects exposed to this kind of proof, I saw the pulsations of the dorsal vessel cease by little and little, and the humor contained in it coagulate. It was only after this coagulation that the membranes of the dorsal vessel absorbed the coloured liquids, and even much more rapidly than the other organs. This vessel thus coloured formed along the back a bluish or reddish line, which had no ramification whatever; but the white fibrillæ of the tracheæ were always distinguishable, which, as has been said, form the first membrane. In this absorption the tracheæ do not sensibly change their colour. They merely assume a deeper tint; but they sink down completely. The adipose tissue which surrounds the dorsal vessel assumes the form of clots. The muscles separate in such a manner that their fibres become perfectly distinct, and that their attachments may be easily recognized. The liquid being at last evaporated, the indigo or carmine appears disseminated between the adipose membranes; but not in an uniform manner, as would be the case if these colouring matters were contained in real vessels.

In other attempts which I made I saw the membranes of the tracheæ absorb the colouring matter; but the absorption was so indistinct, that I merely mention it to put those on their guard who may wish to repeat these experiments. In the locusta I perceived often that the absorption by the membranes of the adipose tissue belonging to the muscular fibres, called wings by Lyonnet, was almost as great as that by the dorsal vessel. It is evident that in this last circumstance the coloured line formed by that vessel ought to be less perceptible. On examining these membranes with a strong glass, we always see distinctly the colour forming a continued line in the dorsal vessel, a continuity which we never see at the side nor in the membranes.

These first attempts being insufficient to detect the ramifications of the dorsal vessel, I injected into it, and made it absorb liquids capable of acting chemically on the humour which it contained. Tincture of nutgalls strongly coloured with indigo and carmine, and filtered, in order to have as few undissolved particles of colouring matter as possible, was the first re-agent which I employed. This liquid, forced into the dorsal vessel by an injecting syringe with a capillary opening, made its way to the extremity of the vessel. I repeated this injection at different times on the larvæ of the geotrupa nasicornis and great butterfly (*paon*), and likewise on the mole crickets (while alive); but never perceived the coloured liquid to make its way into the smallest branch of the dorsal vessel.

I then repeated these injections in a manner somewhat different; and to be certain that they would penetrate into the dorsal vessel, I introduced a globule of fat into this vessel; and in proportion as I pushed the injection, the globule advanced in the interior of that organ. Thus by little and little it came to its extremity, and then I could have no doubt of the success of my injection. As I did not perceive ramifications, I dissected with great care the membrane which surrounds the dorsal vessel, endeavouring to separate it as completely as possible. Passing then a little filtering paper on the side of the vessel, I compressed it gently, in order to see whether any coloured liquid would issue out; because by the preceding dissection I had of necessity cut the openings of the ramifications if they existed. Whatever care I applied to these experiments, varying them, and repeating them frequently, I never perceived any liquid issuing out. Sometimes, when my assistant did not close completely the inferior opening of the dorsal vessel, some portions of coloured liquor made its escape; but it was always at that extremity.

Finally, having made the tincture of nutgalls to be absorbed by the membranes of the dorsal vessel (the insects being alive), I saw the fat and the humour of the vessel coagulate, and then absorb the

Fig. 4.

Fig. 1.

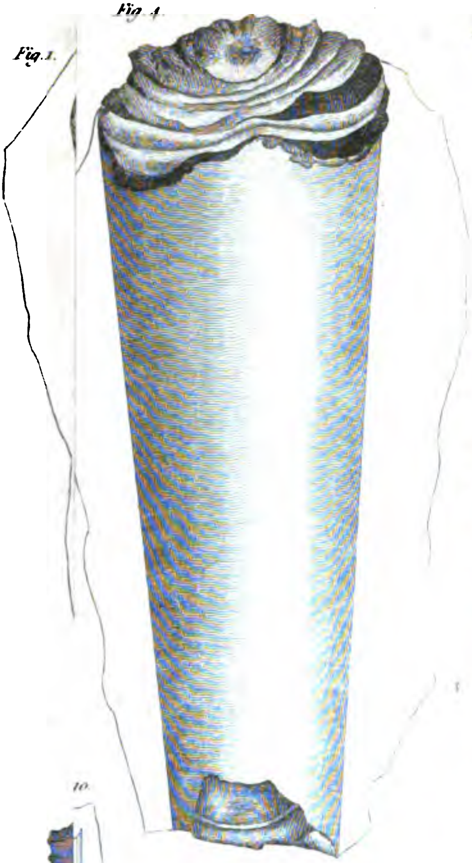


Fig. 5

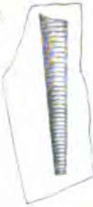
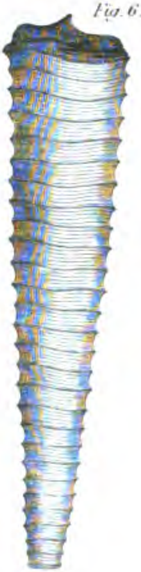
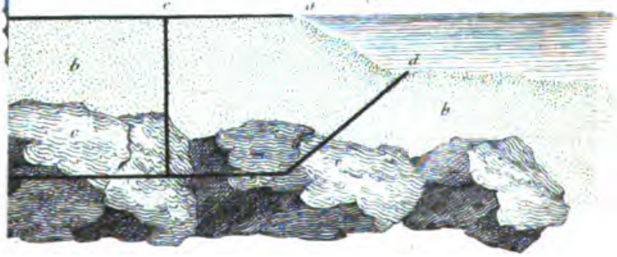


Fig. 6.



10.

Fig. 11



hury sculp.

alcohol; but it was never possible to perceive the least lateral ramification. So constant an agreement in researches so delicate seemed sufficient to lead to the conclusion that the dorsal vessel of insects has no ramifications; but as I had not attempted every thing which it seemed possible to do to ascertain the point, I continued my researches.

The acids appearing to me proper to coagulate the humour of the dorsal vessel, I put some drops of nitric acid on this vessel. The contraction of the organ immediately ceased, but the life did not appear injured, and the insect continued to agitate its members as before. The dorsal vessel, and the humour which it contained, became whitish; and as the alteration gradually extended itself to the muscles, they also became whitish, but with a pearl-coloured lustre quite peculiar. The dorsal vessel filled with a coagulated liquor has thus become more easily observable. On lifting it up with precaution, I did not perceive the smallest fibrilla issuing from it, excepting the tracheæ which pass into it. It is always easy to distinguish these organs, because the nitric acid does nothing else than increase their lustre. I afterwards tried the action of various other acids, and always obtained the same results; only they appear more speedily when sulphuric acid of 69° is used. The action of acids is advantageous to enable us to follow the direction of the muscles, as they communicate to these organs a brilliant pearl colour, and render visible the bundles of which they are composed.

Injections made with solutions of tannin have always shown me the dorsal vessel without any kind of ramification. The same was the case when the solution was absorbed by the membranes of the vessel.

(*To be continued.*)

Article VI.

Observations on the Orthoceratites of Scotland.
By John Fleming, D.D. F.R.S.E.

(Communicated to the Wernerian Natural History Society, May 21, 1814.)

The examination of the history of petrified bodies would be greatly facilitated by the adoption of some regular plan, calculated to give arrangement to the information already obtained, and to fix the attention on those objects which ought to be regarded as of the greatest importance. Thus our knowledge of this branch of science would very rapidly increase, were we to examine with care the characters of the different species, investigate the changes which they have undergone in the mineral regions, and ascertain the various rocks in which they are imbedded, and the relation of these rocks to the strata which constitute the crust of the globe.

By attending to the first of these objects, or to what may be termed the *systematic history of petrifactions*, we avoid all ambiguous references, and introduce accuracy into our descriptive mineralogy by the formation of a general language. Had this department been studied with greater care, geologists would not have been so frequently perplexed in accounting for the phenomena of nature. How often do we hear it asserted that the plants, corals, and shells, which are found in a fossil state in the rocks of this country, bear the strongest resemblance to those of Africa and India? Yet when these are subjected to a close examination, they are found to be specifically distinct, and the distracted philosopher is saved the trouble of deluging the earth by a comet, or of changing its axis of motion. Naturalists were long employed in searching for the means which transported the monsters of the equatorial forests to the frozen regions of the north, until the systematic accuracy of a Camper and a Cuvier proved the fossil elephant to be a new species, differing in form and character from the elephants of Africa or India. Before the physical distribution of petrifactions can be investigated with success, the particular species must be previously ascertained.

The second object deserving attention in the history of petrifactions is the examination of their present condition, with the view of ascertaining the various changes which they have undergone. This may be termed the *chemical history of petrifactions*, as it makes us acquainted with the operations performed in the great laboratory of Nature, and enables us to comprehend the intricate process of mineralization.

The third object of importance in the history of petrifactions is their situation, the strata in which they are imbedded, the particular formations in which they occur, and the organic remains with which they are associated. This may be termed the *geognostic history of petrifactions*, and will be carefully studied by those who are anxious to trace the revolutions which the surface of the earth has undergone, and the various catastrophes which have befallen the former tribes of animals and vegetables. This arrangement will be adopted in the following paper, after having made a few preliminary remarks.

Numerous species of spiral multilocular shells belonging to the genus *nautilus* of Linnæus have been found in a fossil state in the strata of Britain; but the mineralized remains of the straight shells forming the second family of the Linnæan nautili, and included in the modern genus *orthocera*, are of rare occurrence. Lhwydd, in his Ichnographia Lythophylacii Britannici, p. 89, informs us that they are found in Gloucestershire, Oxfordshire, and Northamptonshire. He describes them under the title *alveolus*, and appears to have obtained them from alluvial strata. The late Rev. David Ure, in his History of Rutherglen and Helbrida, characterizes two species of orthoceratites as occurring in the Independent Coal Formation of Lanarkshire. Martin, in his Petrifactions of Derbyshire, mentions their occurrence in the lime-stones of that county; and in the third volume of the *Annals of Philosophy* Dr. Thomson, on the authority of Mr. Greenough, informs us that the orthoceratite occurs in Ireland in rocks decidedly belonging to the floetz formation.

In addition to these notices of the occurrence of the orthoceratite in the strata of this country, I have the satisfaction of stating to the Society that my cabinet of petrifactions contains ten species from the rocks of Linlithgowshire. These are not all to be considered as new species, and therefore interesting to the conchologist, as it is probable that many of them have been described in the works of foreign mineralogists. But they may be regarded as of some importance by the geologist, since they were all found by myself in strata belonging to the Independent Coal Formation of Werner. In order to elucidate this subject as much as possible, I shall give a figure and description of the different species, that their existence in other districts, or in other strata, may be ascertained by comparison.

I.—SYSTEMATIC HISTORY.

Orthocera.—Shell straight or slightly bent, subconical, formed into chambers by simple transverse positions perforated by a tube.

This genus may be conveniently divided into three families, distinguished by the external aspect of the shell.

A.—*Surface of the Shell Smooth.*

The shells belonging to this family are regular in their form, and the surface is smooth and even; but when the shells are worn, and the external covering removed, they appear as if ribbed across, owing to the partitions of the chambers becoming visible.

1. *O. Lævis.*—Shell, conical; partitions, waved; chambers, large; pipe, small and central. Plate XXXI. fig. 1.

The length of the specimen which I possess is upwards of three inches. Its breadth at the base is nine-tenths, and at the apex two-tenths, of an inch; the shell is very thin; the chambers are about two-tenths of an inch in depth; the partitions are waved on both sides, and convex; the pipe is central; and in the middle of the shell, where it is most apparent, it is about the fortieth of an inch in diameter.

The shell is now in the form of a yellowish-white soft lime-stone; the inside is filled with granular lime-stone; but the pipe contains the same kind of lime-stone in which the specimen is imbedded.

This is probably the shell referred to by Ure in his History of Rutherglen, p. 306, pl. xvi. fig. 3, as the orthoceratites superficie lavi. In some of the specimens which he found, the original shell still remained of a horny appearance, very thin, and well polished within.

2. *O. Pyramidalis.*—Shell, tapering; partitions, slightly waved; chambers, large; pipe, small and central. Fig. 2.

This species bears a very close resemblance to the preceding in its structure; but the form of the shell is somewhat different, being much longer in proportion to its breadth; the length of the specimen is upwards of six inches; the diameter at the largest end is one inch and two-tenths, and at the apex two-tenths; the last formed chambers are nearly three-tenths of an inch in depth, while the oldest, towards the point, are scarcely one-tenth. A fragment of this species, found contiguous to the one described, and probably a portion of its inferior extremity, measured two inches in diameter.

3. *O. Cylindracea.* — Shell, nearly cylindrical; partitions, slightly waved; chambers, numerous; pipe, minute and central. Fig. 3.

The largest specimen I possess is about three inches and a quarter in length, four-tenths of an inch in diameter at the base, and three-tenths at the apex; the chambers are scarcely one-tenth in depth. When the outer covering of the shell is removed, the chambers appear very distinct, with a flat surface and a small groove in the place of the partitions.

4. *O. Convexa.*—Shell, nearly cylindrical; partitions, thin and convex; pipe, large and lateral. Fig. 4.

The length of this specimen is four inches and a half, the diameter at the base one inch and a quarter, and at the apex upwards of eight-tenths of an inch; the partitions are remarkably thin, and more convex than any of the preceding; the lower chambers are about an eighth of an inch in depth; the pipe is about three-tenths in diameter, and placed about half way between the centre and the margin.

5. *O. Attenuata.*—Shell, tapering; partitions, nearly circular; chambers, large. Fig. 5.

The shell of this species, in one specimen, is very thin, transparent, and glossy, and in some places it is minutely striated across. The length of another is upwards of six-tenths, the breadth at the base about one-eighth, and at the apex about one-twentieth, of an inch. This space contains 15 chambers.

B.—*Surface of the Shell with Transverse Ridges.*

6. *O. Sulcata.*—Shell, tapering; ridges, waved and striated; pipe, small and central. Fig. 6.

The length of the specimen is two inches and six-tenths, the diameter at the base seven-tenths, and at the apex two-tenths, of an inch. It contains 31 ridges, which are twice waved in going round the shell. Both the ridges and intervening grooves are finely striated.

I consider this as the *orthoceratites superficie sulcata* of Ure, pl. xvi. fig. 2. The waving of the ridges he does not mention; but he observes, that the shell "is finely striated in the same direction with the *sulci*."

7. *O. Undala.*—Shell, tapering; ridges, waved and smooth; pipe, small and central. Fig. 7.

This specimen is one inch and two tenths in length, seven-tenths in diameter at the base, and upwards of five-tenths at the apex; the ridges are 11 in number, more deeply waved, and less numerous, than the preceding species; there is the appearance of an epidermis, of a black colour, and obscurely striated; where the shell is exposed, both the ridges and the grooves are perfectly smooth. An imperfect specimen of an *eschara* adheres to the shell.

8. *O. Annularis.*—Shell, subcylindrical; ridges, distant, nearly even, and smooth. Fig. 8.

The largest portion of this shell which I possess is about an inch in length, and upwards of three-tenths of an inch in diameter; the ridges are nearly one-eighth of an inch asunder, and are more obtuse than the preceding. In the spaces between the ridges there are at least two chambers. I have not been able to observe the pipe, as all the specimens are much incorporated with the lime-stone in which they are imbedded.

9. *O. Rugosa.*— Shell, subcylindrical; ridges, waved and tuberculated, with longitudinal lines; pipe, minute and lateral. Fig. 9.

The length of this specimen is upwards of an inch and a quarter, and the breadth about six-tenths. The ridges are formed into knobs by faint longitudinal lines, which cause the intermediate spaces to exhibit a grooved appearance; they are upwards of two-tenths of an inch asunder, and contain two chambers in the interval; the pipe is very small, and placed close by the edge.

C. *Surface of the Shell with Longitudinal Planes.*

10. *O. Angularis.* — Shell, nearly cylindrical, angular, with about 16 smooth longitudinal planes; pipe, small and lateral. Fig. 10.

I possess about half an inch of this shell, which is nearly of equal thickness, scarcely exceeding the tenth of an inch in diameter.

There is a portion of a *trilobite* adhering to the lime-stone in which this shell is imbedded. It resembles in its general appearance the species from Dudley, in Shropshire, figured in Parkinson's Organic Remains, vol. iii. tab. xvii. fig. 11.

II.—CHEMICAL HISTORY.

The original shell appears still unchanged in species 5, 6, and 7, of a thin and very delicate texture; but in the remaining species the original structure is completely altered. In general, the shell and the partitions are changed into fibrous or granular lime-stone, the chambers occupied with crystals of quartz and lime-spar, and the pipe filled with the matter of the surrounding rocks. This is the case with all the perfect multilocular testacea which I have had an opportunity of examining in a petrified state. The pipe, having an external opening, must have been filled at the period of the deposition of the rocks. The shell and the partitions, on the other hand, have been altered, and their particles have assumed a new and more crystalline arrangement, intimating a previous state of fluidity. The cavities of the chambers, having no external opening, are filled with various crystallized substances, which must have entered in a state of solution through the pores of the shell or the partitions.

When the shell is destroyed, or the partitions broken, then the chambers are found filled with the matter of the surrounding rock. The cavities of the chambers of the *O. sulcata* and *O. undata* are filled with clay-iron-stone. One specimen of the *O. cylindracea* is imbedded in a *septarium*, the base of the shell reaching to the centre of the mass, and the apex projecting beyond its circumference. That part of the shell situated in the external covering, and extending to about five-eighths of an inch, is perfectly regular and entire; but near the base of the shell, and towards the centre of the septarium, the partitions of the chambers are broken, and the cavities filled with clay-iron-stone and iron-pyrites.*

III.—GEOGNOSTIC HISTORY.

The species of the genus orthocera, which have now been described, were found in the strata of the county of Linlithgow, a district of country entirely composed of rocks belonging to the Independent Coal Formation of Werner: species 5, 6, and 7, I obtained from a bed of slate-clay at the Blackburn Colliery, in the parish of Livingstone. This bed alternates with white sand-stone, slate-coal, and compact lime-stone. The slate-clay and the lime-stone contain entrochites, and other spoils of the Ocean; while the beds of sand-stone and coal contain the remains of vegetables. Such strange arrangements of organic remains are not uncommon in the coal-field of the Lothians. One specimen of species 3, included in the septarium, was found in this bed of slate-clay; but other specimens of the same shell, and the remaining species, were found at different places in the middle of the county, imbedded in the strata of lime-stone which traverse the high grounds to the south of Linlithgow.†

In the whole extent of these strata no remains of the belemnite have yet been observed. These must have existed had the orthoceratite been merely the concamerated alveolus of that curious petrifaction. The belemnite, we may add, is one of the rarest fossils in Scotland. Professor Jameson observed it in lime-stone connected with sand-stone and basalt in the Island of Mull; and Mr. Neill found it in slate-clay covered with columnar green-stone in the Shiant Islands; but no other localities have hitherto been mentioned.

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* If this septarium was in a state of fusion at its formation, in what manner was the shell of this orthoceratite preserved from incorporating with the melted mass, and enabled to retain its position?

† The characters of this lime-stone are taken notice of in my account of the Old Silver Mine in Linlithgowshire, inserted in the *Annals of Philosophy*, p. 119 of this vol.

It appears to be the opinion of Werner that different rock formations can be discriminated by the petrifactions which they contain. How far this opinion may hold true with respect to the petrifactions of Germany, we have not the means of ascertaining; but in this country it cannot be considered as the expression of a general law in the distribution of organic remains. In the *transition lime-stone* which occurs between the Crook and Noblehouse * there are a few impressions of bivalve shells, so closely resembling in appearance the shells found in the floetz lime-stones as to lead to the conclusion that they belong to the same species. In the lime-stone connected with the *old red sand-stone* in the Island of Arran there is a particular species of the genus *productus* of Sowerby, which is one of the most common petrifactions in the lime-stones of the Independent Coal Formation of the Lothians and Fife. If we descend from classes and formations to the individual members of a group, it will be found that the remains of the same species of vegetable are distributed through beds of clay-iron-stone, sand-stone, lime-stone, slate-clay, and slate-coal, as I have frequently observed.

The occurrence of the genus of shells which we have been considering in beds of lime-stone, is regarded by some as a proof of such lime-stones belonging to the transition class of rocks. Thus Von Buch, in his Travels through Norway, appears to have referred the lime-stones of that country to the transition class, merely from the circumstance of their containing the remains of orthoceratites. In describing the mineralogical appearances which presented themselves in the neighbourhood of Christiana, where he had discovered rocks of the transition class, he says, "How great was my joy when, at the steep falls of Aggers Elv, above the lower saw-mills, I discovered the orthoceratites, which so particularly distinguish throughout all Europe this formation (transition lime-stone), and this formation alone. They are many feet in length, divided into compartments, and for the most part at the edge and the walls of the compartment changed into calcareous spar. They are by no means unfrequent; several of them generally lie in various directions through one another. Pectinites, and several other not very distinguishable petrifactions, appear frequently between them." English Trans. p. 47.—In the transition lime-stones of this country no orthoceratites have been found. They occur in beds of slate-clay and lime-stone of a more recent period. Thus it appears that the distinguishing character of a rock formation in Norway and Germany is not applicable to the same formation in Britain.

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* See Professor Jameson's Mineralogy of Dumfriesshire, p. 77.

The same species of organic remains which occur imbedded in different strata of the floetz class, appear to have sustained the shock of several revolutions before the total destruction of their tribe; and we may also suppose that part of the race of orthoceratites survived the period of the deposition of the transition class, but became at last enveloped in the beds of the coal formation.

I cannot conclude these observations without taking notice of the great difference in point of size between the existing species of the genus orthocera and those orthoceratites which are found in the mineral regions. The living species of the genus to be met with on the British shores do not exceed a quarter of an inch in length, and required the aid of the microscope for their examination. Only one species has been found in Scotland, the *O. linearis.* (Testacea, Brit. Tab. 30, fig. 9.) It was found by Mr. Montagu in a parcel of minute shells sent to him from Dunbar. It is only a quarter of an inch in length; and its breadth is about one-eighth of its length.

Manse of Flisk, Fifeshire.

Article VII.

On Deepening, Cleaning, Excavating, and Removing Obstructions that prevent Vessels from entering Harbours. By John Rook, jun. of Akehead, Wigton, Cumberland.

When we examine the force and power of water, we are convinced that it is an engine capable of performing various powerful operations. In its course from the uplands, and through the lowlands, till immersed in the Ocean, it forms for itself spacious channels and deep pools. When man brings its gravitating motion under subjection he obtains a useful and faithful servant, that performs for him numerous important labours. Passing from these reflections to a new modification of the use of that element, there seems to be a probability that it might be rendered useful in the improvement of the entrance of harbours, &c. by means of carrying it through tunnels, formed according to the circumstances of situations. These tunnels or pipes might be constructed either by collected materials, such as stone, wood, and metal, or formed by sinking down to the rock, and directing the tunnel in it according to the object in view. The use to which I would apply the principle is that of passing through these tunnels or pipes as powerful a stream of water as could be obtained; by which means considerable quantities of sand, gravel, &c. might be removed by the violent stream of water issuing forth from its confined channel.

Thus Plate XXXI. fig. 11,

a, a, the river flowing into the sea.
b, b, b, sand or gravel, being the object of removal.
c, c, c, the rock.
d, d, d, the tunnel formed in the rock by mining.
e, e, e, large augur holes, according to circumstances.

In this way I expect several new harbours might be formed, old ones deepened, cleaned, &c.; and eventually, in all likelihood, many of those dangerous bars, such as Dublin and Linmouth, removed.

Letting the tide flow into a part of a harbour, and then shutting it in by gates, might frequently be resorted to as a means of obtaining a stream of water.

I remain yours truly,
John Rook, Jun.

Akehead, Wigton, Cumberland,
Dec. 20, 1814.

P. S. I should wish to hear the opinion of any engineer upon the above.

Article VIII.

A Memoir on Iodine. By M. Gay-Lussac.

(*Continued from* p. 109.)

Azote does not combine directly with iodine. We obtain the combination only by means of ammonia. It was discovered by M. Courtois. I shall give it the name of ioduret of azote. It has been accurately analyzed by M. Colin, and I shall briefly state from him the circumstances of its formation and its nature.

When ammoniacal gas is passed over iodine, a viscid shining liquid is immediately formed, of a brownish black colour, which, in proportion as it is saturated with ammonia, loses its lustre and viscosity. No gas is disengaged during the formation of this liquid, which may be called *ioduret of ammonia*. It is not fulminating. When dissolved in water, a part of the ammonia is decomposed; its hydrogen forms hydriodic acid; and its azote combines with a portion of the iodine, and forms the fulminating powder. We obtain the ioduret of azote directly by putting iodine in fine powder into a solution of ammonia in water. This indeed is the best way of preparing it; for the water is not decomposed, and seems to concur in the production of this ioduret only by determining the formation of hydriodate of ammonia.

The ioduret of azote is pulverulent, and of a brownish black colour. It detonates from the smallest shock, and from heat with a feeble violet colour. I have often seen it detonate spontaneously when properly prepared. When put into potash, azote is disengaged, and the same products are obtained as when iodine is dissolved in that alkali. The hydriodate of ammonia, which has the property of dissolving a great deal of iodine, gradually decomposes the fulminating powder, while azote is set at liberty. Water itself has this property, though in a much weaker degree, as M. Courtois observed long ago. Thus the elements of ioduret of azote are but very little condensed. It ought to be prepared with great precautions, and should not be preserved.

It would be difficult to determine by direct experiment the proportion of the principles of this compound; but we ascertain them correctly in the following manner:—

We have seen that the ratio of hydrogen to iodine is 1·3268 to 156·21; and as ammonia is composed of

Hydrogen	18·4756
Azote	81·5244

it follows that the ratio of azote to iodine is that of 5·8544 to 156·21; and such is the ratio of the elements of the fulminating compound. If we reduce these elements to volumes by dividing 5·8544 by 0·96913, the density of azote, and 156·21 by 8·6195, the density of the vapour of iodine, we find that the proportion in volume of the elements is one azote and three iodine. We obtain this proportion directly by observing that the vapour of iodine and hydrogen combine in equal volumes; and that in ammonia the volume of hydrogen is to that of azote as three to one.

If we decompose a gramme (15·444 grains) of the fulminating powder, we obtain, at the temperature of 32°, and under the pressure of 30 inches of mercury, a gaseous mixture amounting to 0·1152 litre (7·03 cubic inches, and composed of 0·0864 of the vapour of iodine and 0·0288 of azote. Though this volume be inconsiderable, yet the explosion is very loud, because it is instantaneous. The same difficulty occurs here as in the detonation of the chloruret of azote, and of all the fulminating bodies which are decomposed into simple substances, producing at the same time heat and light. I do not pretend to resolve this difficulty; but is it not possible that the heat and light which make their appearance in these cases is produced by the shock of the gas produced against the air, or any other fluid, as happens when air is compressed or introduced into a vacuum?* Is it in fact necessary to have recourse to heat to communicate elasticity to gaseous substances condensed in a compound, or, which is the same thing, to put their elements in a state of repulsion? Do we not see, on the contrary, a weak electricity destroy the combinations which resist the repulsive force of a very high temperature?* Supposing these conjectures to have some foundation, it will remain to explain why, when we mix equal volumes of water and of a solution of nitrate of ammonia at the same temperature, the thermometer, as I have observed, sinks more than 9°, though there is a notable increase of density. On the supposition that the capacity of bodies for heat is a function of the absolute quantity of heat which they contain, this fact would lead us to admit that the capacity of the solution of nitrate of ammonia is greater than that of its constituents; but this consequence does not appear to be confirmed by experience; therefore the capacity of bodies for heat does not depend solely upon the absolute quantity of heat which they contain.

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* To explain my idea the better, let us conceive a volume of air in the middle of which is a small metallic ball, containing any elastic fluid in a great degree of compression, and at the same temperature with the surrounding fluid. If we suppose the ball suddenly to burst, which will represent a detonation, we shall have heat and light produced. Now in the detonation of ioduret or chloruret of azote the developement of the gas does not appear to me to differ from that of air strongly compressed in our ball.

I return now to the combinations of iodine with the combustibles, or with those oxides which, not being saturated with oxygen, act like combustibles. I have already spoken of the action of iodine on hydro-sulphuric and phosphorous acids. It remains only to speak of its action on sulphurous acid. In the gaseous state this acid has no action on iodine; but when dissolved in water, the addition of iodine occasions a decomposition of that liquid, and sulphuric acid and hydriodic acid are produced. They cannot be separated by distillation; for at the temperature at which hydriodic acid comes over, sulphurous is reproduced. The liquid in the retort, as well as that which condenses in its neck, is strongly coloured by the iodine which the hydriodic acid holds in solution; but when it comes to the receiver, where it finds sulphurous acid dissolved in water, it becomes colourless, and sulphuric acid is reproduced. The sulphites, the sulphureted sulphites, the white oxide of arsenic, and the hydro-chlorate of protoxide of tin, when mixed with iodine, produce also a decomposition of water, and hydriodic acid is formed. Several hydrogenated substances, particularly the essential oils, alcohol, and ether, yield, according to MM. Colin and Gaultier de Claubry, a portion of their hydrogen to iodine, and convert it into acid. (See Ann. de Chim. t. xc.)

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* Chemical phenomena, in my opinion, cannot be explained by heat alone, supposing them to depend solely on the variation of distance which it produces in the particles of these bodies. M. Laplace remarks (Systeme du Monde, 3d edit. ii. 256) that, in order to conciliate planetary attraction with chemical affinity, "we must suppose the dimensions of the particles so small, when compared with the distances between them, that their density is incomparably greater than the mean density of the whole together. A spherical particle, whose radius is equal to the millionth of a metre, ought to have a density more than six thousand thousand millions greater than the mean density of the earth to produce at its surface an attraction equal to gravitation; but the attractive force of bodies considerably surpasses that of gravitation, as they inflect visibly the rays of light, whose direction is not sensibly changed by the attraction of the earth. Hence the density of atoms would prodigiously surpass that of bodies, if their affinities were only modifications of universal gravitation."

Such a supposition appears exaggerated; but let us admit it for a moment, and see whether the diminution of the affinity of a body corresponds with the increase of the distance of its particles produced by heat. Without knowing exactly the cohesion of copper, for example, in a solid and liquid state, we may admit that it is at least a thousand times greater in the former than in the latter state. Let us suppose also, to keep greatly below the truth, that copper in melting increases eight times in bulk. On this exaggerated supposition the distance between the particles of the copper would have only become double; so that the cohesion should have been only four times smaller if it followed the same law as gravitation. Hence it is obvious that, when heat accumulates in a body, it does not diminish the affinity merely by increasing the distance of the particles, but by increasing in a great degree the power of their repulsive faculty, which is doubtless the same with their electric faculty. The figure, the arrangement, and the inertia, of the atoms, may have influence in some chemical phenomena; as, for example, in the congelation of water, and the crystallization of sulphate of soda. But there are an infinity of others which are independent of these, as well as of the separation of the atoms. Such is the combination of hydrogen with oxygen, which takes place only at a red heat, whether the gases be in a condensed or rarified state.

Action of Iodine on the Oxides.

Iodine may be made to act upon the oxides either with or without the presence of water; and as the phenomena are different in the two cases, I shall explain, in the first place, what happens when iodine in the state of vapour is passed over the oxides in a moderate red heat.

The oxide of potassium, produced by the combustion of potassium in oxygen gas, is decomposed by iodine. Oxygen is disengaged, and we obtain ioduret of potassium. It may be shown, by collecting all the products, that the potassium does not retain any oxygen, whatever was its state of oxidation; but I shall give below an easy means of ascertaining this point. Meanwhile I shall state an experiment which shows it in a decisive manner.

I passed vapour of iodine in a red heat over melted subcarbonate of potash; and I obtained carbonic acid and oxygen gases in the proportion of two in volume of the first and one of the second, precisely the proportions which exist in the subcarbonate. The oxide of sodium and the subcarbonate of soda are also completely decomposed by iodine. From these experiments it would seem that this substance ought to disengage oxygen from most of the oxides: but this only happens in a small number of cases. The protoxides of lead and bismuth are the only oxides not reducible by heat which presented me with that property. The protoxides of copper and tin, indeed, absorb iodine; but as the peroxides of these metals do not combine with it, and as no oxygen is disengaged, I conclude that the oxygen of one portion of the protoxide passes into the other portion, and that we obtain a mixture of ioduret and peroxide; so that it is by the concurrence of the forces that these two protoxides are decomposed by iodine.

Barytes, strontian, and lime, combine with iodine without giving out oxygen gas. The oxides of zinc and iron undergo no alteration. From these facts we must conclude that the decomposition of the oxides by iodine depends less upon the condensed state of the oxygen than upon the affinity of the metal for iodine.

The iodurets of barytes, strontian, and lime, are very alkaline when they are dissolved in water. I consider them as subiodurets.

In this respect they resemble the sulphurets of these earths, which have likewise an excess of base.

Having ascertained that the oxides of potassium and sodium are decomposed by iodine, I wished to know if this would happen when they were combined with acids. Sulphate of potash was not altered by iodine; but, what may appear astonishing, I obtained oxygen with the fluate of potash, and the glass tube in which the operation was conducted was corroded. On examining the circumstances of this experiment I ascertained that the fluate became alkaline when melted in a platinum crucible. This happened to the fluate over which I passed iodine. It appears, then, that the iodine acts upon the excess of alkali, and decomposes it. The heat produced disengages a new portion of fluoric acid or of its radicle, which corrodes the glass; and thus by degrees the fluate is entirely decomposed.

The experiments, which I have stated, show that chlorine has more energy than iodine; for M. Thenard and I have shown that chlorine gives out oxygen with barytes, strontian, lime, and even magnesia. I find the same thing happens when it is made to act upon the sulphates of these bases; but what is remarkable, we obtain no oxygen when chlorine is made to act upon the peroxide of iron, because a chloruret of that oxide is formed.

The same experiments furnish a new proof that sulphur has less energy than iodine. In fact, if we obtain metallic sulphurets with most oxides, while we do not obtain iodurets, this depends upon the great affinity of sulphur for oxygen, and upon the gaseous state of sulphurous acid. If iodine formed a gaseous acid with oxygen not decomposable by a high temperature, there can be no doubt that we should be able to form a greater number of metallic iodurets than sulphurets. The decomposition of potash, soda, litharge, and oxide of bismuth, and the formation of iodurets with the protoxides of copper and tin, show this to be the case.

It may be worth while to remark that iodine, like sulphur, has little affinity for oxides; and that, except barytes, strontian, and lime, no oxide can remain in combination with iodine at a red heat.

Very different results take place when iodine and oxides are made to act upon each other in contact with water. The water is then decomposed. Its hydrogen unites with iodine, and forms hydriodic acid; while its oxygen forms with iodine a peculiar acid, to which I have given the name of iodic acid. All the oxides, however, do not give the same results. We obtain them only with potash, soda, barytes, strontian, lime, and magnesia. The oxide of zinc precipitated by ammonia from its solution in sulphuric acid, and well washed, gives no trace of iodate and hydriodate.

Let us examine more particularly what happens when iodine is made to act upon a concentrated solution of potash. In proportion as the iodine dissolves in the alkali, which it does with rapidity, we obtain a granular white precipitate, which melts on burning coals like nitre, and which is decomposed by heat, giving out oxygen and ioduret of potassium. This precipitate is an alkaline iodate of potash. The liquid contains hydriodate. Hence water must have been decomposed;* its hydrogen, uniting with iodine, has formed hydriodic acid, and its oxygen iodic acid.

The potash solution remains slightly coloured; orange yellow, when the alkali is in excess; but it is of a deep reddish brown when the alkali is saturated with iodine. This colour is owing principally to the solution of iodine in the hydriodate of potash. I found that when a solution of a determinate quantity of potash, so weak as not to allow the iodate of potash to precipitate, is entirely saturated with iodine, we must add a quantity of potash equal to the original dose to bring it to the point when its colour is a light-orange-yellow. Even when the solution is saturated with iodine, it is always alkaline; though if we dissolve in water ioduret of potassium or of zinc, we obtain neutral compounds. This difference, which we find in the analogous combinations of sulphur and chlorine, is owing to this, that the forces which tend to decompose water are smaller in the first case than in the second.

Concentrated soda produces, like potash, a fulminating salt, which is partly precipitated, and a hydriodate which remains in solution. Barytes, lime, and strontian, present the same phenomena; with this difference only, that their iodates are very little soluble. This last circumstance enables us to obtain these salts in a state of purity. In order to obtain the iodates of potash and soda free from hydriodates, and perfectly neutral, it is necessary to crystallize them repeatedly, which greatly diminishes their quantity. I shall therefore state the method which I employ to prepare them in order to avoid that inconvenience.

Upon a determinate quantity of iodine I pour solution of potash or soda till the liquid ceases to be coloured. I then evaporate to dryness, and digest the dry salt in alcohol of the specific gravity 0·81 or 0·82. As the iodate is not soluble in this liquid, while the hydriodate is very soluble, the two salts easily separate from each other. After having washed the iodate two or three times with alcohol, I dissolve it in water, and neutralize it with acetic acid. I evaporate to dryness, and digest the dry salt in alcohol, to remove the acetate. After two or three washings, the iodate is pure. As for the alcohol containing the hydriodate, we distil it off, and then neutralize the potash by means of hydriodic acid.

Here it may be asked whether, at the moment that the alkali acts upon the iodine, iodate and hydriodate form and exist separately.

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* I reason on the hypothesis of the existence of hydriodates. But if we do not admit their existence, the oxygen which combines with the iodine, and converts it into an acid, must have been furnished by a portion of the potash.

We shall endeavour to answer this question hereafter. We shall terminate what concerns the action of the oxides on iodine by treating of those in which the oxygen is very little condensed, as the oxides of mercury, gold, and silver.

M. Colin has observed, that when we expose to a heat between 140° and 212° a mixture of water, iodine, and peroxide of mercury, super-iodate of mercury is formed, which is held in solution in the water and an iodate with excess of base, which is insoluble, and remains mixed with the red ioduret which is formed at the same time. Oxide of gold treated in the same manner does not appear to form an ioduret; for after a great number of washings, metallic gold remains, and the water holds an acid iodate in solution. We may suppose that the water has been decomposed, and that the oxides of mercury and gold act upon iodine like the alkalies; but if we consider that the oxide of zinc does not form an iodate, it will become exceedingly probable that the iodic acid has been formed at the expense of the oxygen of a part of the oxide. If we recapitulate the action of the oxides on iodine, it will appear,

1. That the alkaline oxides in which the oxygen is very much condensed, and which completely neutralize the acids, occasion with iodine the decomposition of water, and produce iodates and hydriodates.

2. That the metallic oxides in which the oxygen is still very much condensed, though less than in the preceding, and which do not completely neutralize the acids, do not produce with iodine a force sufficiently great to decompose water and form iodates.

3. That the oxides in which the oxygen is weakly condensed cannot concur with iodine in decomposing water; but that they convert iodine into an acid by yielding to it a portion of their oxygen.

Such are the general results of the action of iodine on the oxides. We shall afterwards treat in detail of some saline combinations of iodine; but at present we propose to state the properties of iodic acid.

We have seen that the acid is only formed by the concurrence of different forces, and that it is always combined with some base, from which, of course, we must separate it. At first sight it seems that we might treat with hydro-sulphuric acid the iodates of easily reducible metals; but the iodic acid would be at the same time decomposed by this acid, because its elements are very little condensed.* After various trials, I adopted the following process:—Upon iodate of barytes I pour sulphuric acid diluted with twice its weight of water, and heat the mixture. The iodic acid quickly abandons a portion of its base, and combines with the water. But although a quantity of sulphuric acid was employed not sufficient to saturate the whole of the barytes in the iodate, a small portion of it always remains mixed with the iodic acid. If we endeavour to separate this portion by adding barytes-water, the two acids precipitate together. I suppose that the presence of a small quantity of sulphuric acid in the iodic acid depends upon the strong affinity of this last acid for barytes. I do not think there is reason to believe that the two acids have a tendency to combine together, in consequence of which the iodate is decomposed.

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* Hydro-sulphuric acid may be employed to decompose phosphate of lead and obtain phosphoric acid.

The iodate of lime and sulphuric acid give similar results. When the same salt and oxalic acid are used, the decomposition seems to be more complete.

Hitherto iodic acid has only been obtained in combination with water, and it is very probable that this liquid is as necessary as a base to keep the elements of the acid united, as we see is the case with sulphuric acid, nitric acid, &c.* Its taste is very acid when it is concentrated. Light does not decompose it. It may be evaporated to the consistence of a syrup. But if its temperature be raised to about 392°, it is decomposed entirely into iodine and oxygen. Sulphurous and hydro-sulphuric acids immediately separate iodine from it. This acid and hydriodic acid are decomposed almost completely, like sulphurous and hydro-sulphuric acids. If we mix it with concentrated hydro-chloric acid, chlorine is disengaged. Sulphuric and nitric acids have no action on it. With solution of silver it gives a white precipitate, very soluble in ammonia. It combines with all the bases, and produces all the iodates which we obtain by making the alkaline bases act upon iodine in water. It likewise forms with ammonia a salt, which fulminates when heated, the existence of which I announced some time ago.

From experiments which I shall state in speaking of the iodates, this acid is composed of

Iodine	100
Oxygen	31·927

Now the first possible combination of iodine and oxygen being

Iodine	100
Oxygen	6·4017

Iodic acid is evidently a compound of

Iodine 100

Oxygen ....6·4017 × 5 = 32·0085

That is to say, that iodic acid contains five proportions of oxygen.

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* The existence of acids without water shows a greater reciprocal affinity in their elements than in those acids which exist only by means of water or a base.

(*To be continued.*)

ARTICLE IX.

Annual Meteorological Table for Sidmouth, Devon, 50° 41′ N. lat. 3° 13′ W. long. By Dr. Clarke.

814.	*Thermometer.—Northerly Exposure.*																*Barometer.*								*Hygrometer of De Luc.*								*Weather*			*Winds.*				*Rain.*
Months	Day	Highest by Six's Register	Wind	Day	Lowest by Six's Register	Wind	Mean of the extremes	Mean of the highest	Mean of the lowest	Mean of highest and lowest	Mean of 9 morning	Mean of 2 afternoon	Mean of 9 A. M. and 2 P. M.	Mean of 11 at night	Mean of 9 A. M. 2 P.M. and 11 P.M.	Greatest variation in 24 hours	Day	Highest	Wind	Day	Lowest	Wind	Mean for the month	Greatest range in 24 hours	Day	Greatest moisture	Wind	Day	Least moisture	Wind	Range in 24 hours	Mean	Fine	Cloudy	Wet	N and N E	E and S E	S and S W	W and N W	Inches and decimals
an.	29	44°	N W	21	18°	N	31°	35°	29°	32°	32°	35°	33°	31°	33°	15°	1	30·21	S E	29	28·50	N W	29·63	·91	—	—°	—	—	—°	—	—°	—°	16	1	14	19	4	2	6	3·85
eb.	5	53	N W	21	23	N E	38	43	33	38	36	42	39	36	38	12	17	30·51	N E	6	29·68	W	30·14	·35	—	—	—	—	—	—	—	—	20	1	7	9	11	3	5	·68
arch	27	54	S W	7	27	N E	40	44	35	39	38	43	40	37	39	10	15	30·50	N E	2	28·96	W	29·86	·29	—	—	—	—	—	—	—	—	17	2	12	13	8	5	5	·75
pril	18	68	N	4	35	N	49	56	43	50	50	54	52	45	50	8	8	30·32	S	2	29·42	S W	29·92	·34	—	—	—	—	—	—	—	—	15	1	14	4	7	11	8	3·25
ay	26	67	S	11	38	E	52	61	44	52	51	57	54	46	51	10	11	30·51	E	5	29·41	S E	30·04	·47	—	—	—	—	—	—	—	—	20	10	1	6	14	9	2	·60
une	30	75	S	7	42	S E	59	66	51	58	60	63	61	52	58	10	24	30·41	E	8	29·76	N E	30·10	·33	—	—	—	—	—	—	—	—	18	1	11	5	9	4	12	2·85
uly	25	76	S E	2	47	S W	61	71	55	63	66	60	67	56	62	9	23	30·36	S E	20	29·74	S	30·04	·25	—	—	—	—	—	—	—	—	20	1	10	1	4	12	14	1·60
ug.	4	76	S	28	46	N	61	70	54	62	63	68	65	55	62	10	31	30·39	S	24	29·70	S W	30·06	·40	—	—	—	—	—	—	—	—	16	1	14	2	2	20	7	1·50
ept.	1	73	S	10	42	S W	57	66	50	58	60	63	62	52	58	9	1	30·41	S	24	29·70	S W	30·13	·27	24	41	S W	10	14	S W	15	35	20	0	10	8	9	10	3	1·60
ct.	14	61	S	9	33	N W	48	55	44	50	49	54	52	46	50	12	10	30·34	N W	19	29·19	S W	29·87	·58	12	40	S	2	14	N E	18	28	17	0	14	1	5	11	14	3·20
ov.	14	57	N	21	25	N W	41	49	39	44	43	47	45	48	44	11	11	30·44	S W	30	29·36	W	29·65	·53	25	39	S	21	25	N W	10	31	15	1	14	8	0	8	14	1·60
ec.	11	57	S W	26	28	N E	42	48	40	44	43	46	44	42	43	17	20	30·31	N W	28	29·12	N E	29·79	·59	30	42	S W	16	25	N W	14	35	12	0	19	5	4	12	10	4·05

ANNUAL RESULTS.

Thermometer.

Highest by Six's Register, July 25....	76°
Lowest by ditto in the night, Jan. 21..	18
Mean of the extremes...............	48
Mean of the mean of the highest......	55
Mean of the mean of the lowest......	43
Mean of these two..................	49
Mean of 9 morning..................	49
Mean of 2 afternoon	53
Mean of 9 morning and 2 afternoon...	51
Mean of 11 night	45
Mean of 9 A.M. 2 P.M. and 11 P.M..	49
Greatest variation in 24 hours, Dec. 19—20	17

Mean Temperatures from Observations taken for	At 9 A. M. 2 P. M. and 11 P. M.	At 9 A.M. and 2 P.M.
Nov. Dec. and Jan.	40	41
Feb. March, and April	43	44
May, June, and July........	58	61
Aug. Sept. and Oct.	57	60
Six winter months...........	42	43
Six summer months	58	61
Annual Mean	50	52

Barometer.

Highest, Feb. 17....	30·51
Lowest, Jan. 29	28·50
Greatest range, Jan. 28—29..	·91
Annual mean	29·95

De Luc's Hygrometer.

Mean for the months .. 30°

Weather.	*Days.*
Fine..........	206
Cloudy........	19
Wet..........	140
	365

Wind.	*Days.*
N and N E......	81
E and S E	77
S and S W......	107
W and N W	100
	365

	Inches.
Rain for the year	25·73

ARTICLE X.

Magnetical and Astronomical Observations at Hackney Wick.

By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North. Longitude West in Time 6″ $\frac{82}{100}$.

Feb. 4, Emersion of Jupiter's 4th Satellite................	12h	22′	55″	Mean Time at Hackney Wick.
	12	23	02	Ditto at Greenwich.
Feb. 7, Immersion of Jupiter's 1st Satellite................	12	05	26·4	Ditto at Hackney Wick.
	12	05	33·2	Ditto at Greenwich.

Magnetical Observations.

1815.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Jan. 18	8h	45′	24°	15′	46″	1h	30′	24°	19′	33″	Not observed.	Not observed.
Ditto 19	8	40	24	16	33	2	00	24	18	33		
Ditto 20	8	50	24	15	50	1	20	24	19	40		
Ditto 21	8	55	24	15	00	1	45	24	19	09		
Ditto 22	—	—	—	—	—	1	25	24	22	01		
Ditto 23	8	50	24	18	20	1	35	24	21	03		
Ditto 24	—	—	—	—	—	1	50	24	20	44		
Ditto 25	8	45	24	15	17	1	25	24	21	50		
Ditto 26	8	55	24	15	22	—	—	—	—	—		
Ditto 27	8	50	24	16	18	1	25	24	21	18		
Ditto 28	8	50	24	16	13	1	30	24	20	13		
Ditto 29	8	45	24	18	06	1	25	24	22	01		
Ditto 30	8	50	24	21	14	1	35	24	21	41		
Ditto 31	8	50	24	17	58	1	50	24	22	03		

1815. Mean of Observations in Jan.	Morning	at	8h	47′.....	Variation	24°	16′	46″	West.
	Noon	at	1	36.....	Ditto	24	20	12	
	Evening	at	—	—.....	Ditto	—	—	—	Not obs.
1814. Ditto in Dec.	Morning	at	8	44.....	Ditto	24	18	02	West.
	Noon	at	1	30.....	Ditto	24	20	36	
	Evening	at	—	—.....	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	41.....	Ditto	24	16	20	West.
	Noon	at	1	40.....	Ditto	24	20	37	
	Evening	at	—	—.....	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	39.....	Ditto	24	14	08	West.
	Noon	at	1	42.....	Ditto	24	21	45	
	Evening	at	—	—.....	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	32.....	Ditto	24	44	33	West.
	Noon	at	1	39.....	Ditto	24	23	17	
	Evening	at	6	19.....	Ditto	24	16	50	
Ditto in Aug.	Morning	at	8	30.....	Ditto	24	14	13	West.
	Noon	at	1	39.....	Ditto	24	23	48	
	Evening	at	6	57.....	Ditto	24	16	31	
Ditto in July.	Morning	at	8	41.....	Ditto	24	13	29	West.
	Noon	at	1	42.....	Ditto	24	23	44	
	Evening	at	6	58.....	Ditto	24	17	00	
Ditto in June.	Morning	at	8	44.....	Ditto	24	13	10	West.
	Noon	at	1	39.....	Ditto	24	22	48	
	Evening	at	6	52.....	Ditto	24	16	29	
Ditto in May.	Morning	at	8	45.....	Ditto	24	13	12	West.
	Noon	at	1	44.....	Ditto	24	22	13	
	Evening	at	6	38.....	Ditto	24	16	14	

Mean of Observations in April.	Morning	at	8h	45′	..... Variation	24°	12′	53″	West.
	Noon	at	1	48	..... Ditto	24	23	53	
	Evening	at	6	29	..... Ditto	24	15	30	
Ditto in March.	Morning	at	8	52	..... Ditto	24	14	29	West.
	Noon	at	1	52	..... Ditto	24	23	08	
	Evening	at	6	11	..... Ditto	24	15	33	
Ditto in Feb.	Morning	at	8	47	..... Ditto	24	14	50	West.
	Noon	at	1	52	..... Ditto	24	20	58	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	52	..... Ditto	24	15	05	West.
	Noon	at	1	53	..... Ditto	24	19	03	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at	8	53	..... Ditto	24	17	39	West.
	Noon	at	1	51	..... Ditto	24	20	30	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	42	..... Ditto	24	17	17	West.
	Noon	at	1	54	..... Ditto	24	20	24	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	..... Ditto	24	15	41	West.
	Noon	at	1	59	..... Ditto	24	22	53	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	..... Ditto	24	15	46	West.
	Noon	at	2	02	..... Ditto	24	22	32	
	Evening	at	6	03	..... Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	..... Ditto	24	15	55	West.
	Noon	at	2	02	..... Ditto	24	23	32	
	Evening	at	7	05	..... Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	..... Ditto	24	14	32	West.
	Noon	at	1	50	..... Ditto	24	23	04	
	Evening	at	7	08	..... Ditto	24	16	43	
Ditto in June.	Morning	at	8	30	..... Ditto	24	12	55	West.
	Noon	at	1	33	..... Ditto	24	22	17	
	Evening	at	7	04	..... Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	..... Ditto	24	12	02	West.
	Noon	at	1	37	..... Ditto	24	20	54	
	Evening	at	6	14	..... Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	..... Ditto	24	09	18	West.
	Noon	at	0	59	..... Ditto	24	21	12	
	Evening	at	5	46	..... Ditto	24	15	25	

Magnetical Observations continued.

1815.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Feb. 1	8h	45′	24°	17′	36″	1h	30′	24°	20′	27″	Not observed.	Not observed.
Ditto 2	8	45	24	15	52	1	30	24	20	10		
Ditto 3	8	40	24	15	43	—	—	—	—	—		
Ditto 4	8	40	24	18	15	1	25	24	21	48		
Ditto 5	8	30	24	14	20	1	30	24	21	26		
Ditto 6	8	55	24	14	42	1	45	24	22	03		
Ditto 7	8	45	24	15	11	1	40	24	19	37		
Ditto 8	8	45	24	16	40	1	25	24	22	23		
Ditto 10	8	35	24	15	13	1	40	24	21	49		
Ditto 11	8	40	24	15	15	1	30	24	23	16		
Ditto 12	8	45	24	15	14	1	35	24	23	12		
Ditto 13	—	—	—	—	—	1	30	24	22	25		
Ditto 14	8	20	24	15	07	—	—	—	—	—		
Ditto 15	8	40	24	14	06	1	40	24	22	47		
Ditto 16	8	55	24	14	36	—	—	—	—	—		
Ditto 17	8	30	24	14	12	1	30	24	22	42		

Comparison of the Variations in the Years 1813, 1814, *and* 1815.

		1813.			1814 and 1815.			Difference.		
April	Morning	24°	09′	18″	24°	12′	53″	+	3′	35″
	Noon	24	21	12	24	23	53	+	2	41
	Evening	24	15	25	24	15	30	+	0	05
May	Morning	24	12	02	24	12	49	+	0	47
	Noon	24	20	54	24	22	13	+	1	19
	Evening	24	13	47	24	16	14	+	2	27
June	Morning	24	12	35	24	13	10	+	0	35
	Noon	24	22	17	24	22	48	+	0	31
	Evening	24	16	04	24	16	29	+	0	25
July	Morning	24	14	32	24	13	29	–	1	03
	Noon	24	23	04	24	23	44	+	0	40
	Evening	24	16	43	24	17	00	+	0	17
Aug.	Morning	24	15	55	24	14	13	–	1	42
	Noon	24	23	32	24	23	48	+	0	16
	Evening	24	16	08	24	16	31	+	0	23
Sept.	Morning	24	15	46	24	14	33	–	1	13
	Noon	24	22	32	24	23	17	+	0	45
	Evening	24	16	04	24	16	50	+	0	46
Oct.	Morning	24	15	41	24	14	08	–	1	33
	Noon	24	22	53	24	21	45	–	1	08
	Evening	—	—	—	—	—	—		—	—
Nov.	Morning	24	17	17	24	16	20	–	0	57
	Noon	24	20	24	24	20	37	–	0	13
	Evening	—	—	—	—	—	—		—	—
Dec.	Morning	24	17	39	24	18	02	+	0	23
	Noon	24	20	30	24	20	36	+	0	06
	Evening	—	—	—	—	—	—		—	—
Jan.	Morning	24	15	05	24	16	26	+	1	41
	Noon	24	19	03	24	20	12	+	1	09
	Evening	—	—	—	—	—	—		—	—

In deducing the mean of the observations, that of the morning of Jan. 30 is rejected, the variation exceeding that of any former day, without any apparent cause.

Jan. 18.—The needle in the morning unsteady, and followed by a fall of snow. During the fall the needles were quite steady.

Jan. 27.—Needles unsteady at noon: the wind afterwards blew fresh from the S.E., accompanied with snow.

Feb. 12.—The needles at noon vibrated 14′ 40″ at intervals. The wind blew strong from the S. W. The wind afterwards increased, and shifted to the westward, accompanied with hard rain and showers of hail.

Rain fallen	Between noon of the 1st Jan. Between noon of the 1st Feb.	0·694 inches.

ARTICLE XI.

ANALYSES OF BOOKS.

A Voyage to Terra Australis, undertaken for the Purpose of completing the Discovery of that vast Country, and prosecuted in the Years 1801, 1802, *and* 1803, *in his Majesty's Ship the Investigator; and subsequently in the armed Vessel Porpoise, and Cumberland Schooner. With an Account of the Shipwreck of the Purpose, arrival of the Cumberland at the Mauritius, and Imprisonment of the Commander during six Years and a half in that Island. By Matthew Flinders, Commander of the Investigator. In two Volumes, with an Atlas. London,* 1814.

THE charts which accompany this work have been constructed with uncommon care, and are probably the most complete of their kind hitherto published. The workmanship does great honour to Captain Flinders, and to the artists employed.

Captain Flinders, by way of introduction to his own voyage, gives a preliminary account of the previous discoveries made in the Terra Australis by preceding voyagers. The north coast of New Holland appears to have been discovered in 1606, by the Duyfhen, a Dutch yatch, dispatched from Bantam to explore the islands of New Guinea. But the commander of this vessel was not aware of the importance of his discovery, considering the country which he observed as a part of New Guinea. Torres, a Spanish navigator, saw it the same year; but was as little conscious of the importance of his discovery. His letter to the King of Spain remained unknown, till a copy of it was found when Manilla was taken by the British forces in 1762. Mr. Dalrymple made it known to the public, and gave the name of Torres to the strait on the north-east side of New Holland, through which that navigator appears to have passed; and this name has been generally adopted. Further progress was made in the investigation of the north coast, by John Carstens, in 1623, by Gerrit Tomaz Pool and Pieterz Pieterzen, in 1636, and by three Dutch vessels, in 1705.

In 1770, Captain Cook sailed along the north coast, and ascertained that New Guinea is not connected with New Holland. In 1791, Lieutenant M'Lure sailed along the north coast of New Holland, and determined the position of several islands, shoals, and projecting points of land. These were all the navigators that preceded Captain Flinders in his examination of the north coast. But Captains Bligh and Portlock, in 1792, and Messrs. Bampton and Alt, in 1793, sailed through Torres Straits, and added considerably to our knowledge of that intricate navigation.

The west coast of New Holland appears to have been first seen by Dirk Hartog, commander of the ship Endragt, outward-bound from Holland to India. He fell in with it in 1616, about latitude

26½°, south, and coasted along it to latitude 23°. Edel, another Dutch Captain, fell in with that part of the coast which bears his name, in 1619. It was seen by the ship Leeuwin in 1622, and by the ship Vianen in 1628. In 1629, the Batavia, commanded by Francisco Pelsert, was wrecked on the coast of New Holland, on the rocks called Abrolhos. Captain Pelsert coasted along the shore a considerable way in a boat, and then sailing to Batavia, returned in another vessel, to take off the people left upon the Abrolhos rocks. Tasman is supposed to have examined part of the north-west coast of New Holland in 1644. The buccaneers, with whom Dampier made a voyage round the world in 1688, came upon the north-west coast of Terra Australis, for the purpose of careening their vessel and procuring refreshments. They made the land about latitude 16° 50′, due south from a shoal whose longitude is now known to be 122¼°, east. From this place they ran along the shore N. E. by E. twelve leagues, till they came to a convenient place for their purpose. Dampier gives a particular description of the country and inhabitants. Vlaming, another Dutch Captain, surveyed a considerable portion of the west coast in 1696, being ordered to look out for the crew of a vessel which had been lost the year before, and which it was supposed might have been wrecked on that coast, and the crew saved. In 1699, Dampier visited the west coast of New Holland a second time, having been sent out in purpose on a voyage of discovery in his majesty's ship Roebuck. These are all the navigators that preceded Captain Flinders on the west coast.

The south coast of New Holland was discovered by Pieter Nuyts, a Dutchman, in 1627, and the coast was accurately laid down by him, from Cape Leeuwin to about east longitude 132°. The next person who visited it was Captain Vancouver, in 1791. He made the coast at Cape Chatham, in latitude 35° 3′ south, and longitude 116° 35′ east. He sailed along the coast east, and anchored in a sound to which he gave the name of King George the Third. Thence he sailed east as far as Termination Island, in east longitude 122° 8′. The French Admiral, D'Entrecasteaux, who was sent out in search of La Peyrouse, visited the south coast of New Holland in 1792; but did not proceed further east than Nuyts had done. His charts, as far as he went, are accurate and satisfactory. Thus 250 leagues of the south coast remained entirely unexplored when Captain Flinders commenced his voyage.

Van Dieman's Land was discovered by Tasman, in 1642, who was sent out by the Dutch expressly to determine how far south the continent of New Holland extends. Captain Cook sailed along the east coast of that continent in 1770, and ascertained the general outline of the coast. Morion, a French naval officer, visited Van Dieman's Land in 1772, Captain Furneaux in 1773, Captain Cook in 1777, and D'Entrecasteaux in 1793; but none of these navigators had been able to decide whether Van Dieman's Land was an island or part of the continent. After the British colony was established at Port Jackson, various expeditions were sent from that place to ascertain the east coast of this country with precision; and in these expeditions, Captain Flinders, and Mr. Bass, surgeon of the Reliance, cut a most conspicuous figure. These two enterprizing gentlemen sailed from Port Jackson in 1795, in a small boat only eight feet long, called the Tom Thumb, with a crew consisting only of themselves and a boy. They explored about 35 miles of coast, and ascertained the figure and value of several bays and openings. In 1797, Mr. Bass set out in a whale boat, with a crew of eight picked men, to explore the east coast. He sailed south, was absent above eleven weeks, and examined above 600 miles of coast; entering Bass's Straits, and making considerable progress in them, and satisfying himself that Van Dieman's Land was an island; though the evidence which he obtained was not sufficiently strong to satisfy others. In 1798, Captain Flinders and Mr. Bass were sent on purpose from Port Jackson, to determine the point. They sailed through Bass's Straits and round Van Dieman's Land, and thus put it beyond doubt that Van Dieman's Land is an island. Such was the state of our knowledge of the coast of New Holland when Captain Flinders's voyage was undertaken in 1801.

Captain Flinders sailed from Spithead on the 18th of July, 1801, in the ship Investigator, of 334 tons burthen, with a compliment of 88 men. The vessel had been a collier, and though old, was the best which the Admiralty could at that time spare for this service. After touching at Madeira, and stopping some time at the Cape of Good Hope, he made the coast of New Holland on the 7th of December. The part of the coast which he first saw was Cape Leeuwin, the south western and most projecting part of Leeuwin's Land. The highest hill lies nearly in latitude 34° 19', south, and longitude 115° 6', east. From this port he coasted along the shore till he came to King George the Third's Sound, where he put the ship in order, and took in wood and water. Bald Head, the entrance to this sound, lies in latitude 35° 6' 15", south; and in longitude 118° 0' 45", east. It was from this point that he was instructed to make a minute survey of the south coast of New Holland as far as Bass's Strait.

From King George the Third's Sound to about east longitude 131, a space of 15 degrees of longitude, the coast runs easterly with a slight inclination to the north; and the whole of this space consists of a cliff between 5 and 600 feet high, the lower part of which is white and the upper part brown. Not a single river runs into the sea in all this vast tract, nor was there a single landing place to be seen. Captain Flinders conceives that this long cliff may be a coral reef, elevated by some unknown means so much above the level of the sea; and thinks it not unlikely that an island, sea, or lake, may be on the other side of the cliff. It is much to be regretted that this conjecture was not verified by an attempt to climb the cliff, and see what was on the other side of it. About longitude 131, which is the furthest north point of the south coast of New Holland, the shore becomes low, and constitutes a sandy beach. Thus far east the south coast had been surveyed by Admiral Entrecasteaux.

From this place the coast assumes a south-easterly direction, is usually sandy and low, but rises gradually as the land recedes from the sea. There was no appearance of any river, though several bays occurred, and different archipelagos of small islands were passed, as Nuyts, Investigators, &c. At south latitude 35°, and east longitude 136°, they came to a point of land called Cape Catastrophe, because here Mr. Thistle, the master, a midshipman, and six seamen were lost by the oversetting of a boat in the dark. At this point the land tended to the north, and there was every appearance of a large opening, or bay, which Captain Flinders resolved to examine with every possible care.

This gulf, to which he gave the name of Spencer's Gulf, is about 50 miles wide at its entrance, and extends north about 183 miles, from latitude 36° south to latitude 32½°. It gradually narrows towards the north, and where it terminates the water is as salt as out at sea. Yet a good deal of fresh water must be poured into it here at certain seasons. The shore has a barren appearance on the east side; towards the north there is a ridge of mountains, the highest of which, Mount Brown, seems to be about 3000 feet above the level of the sea. The view from this mountain is that of an immense level country covered with wood. At the south side of Spencer's gulf there is a large island called Kangaroo Island, from the great number of kangaroos found on it. The strait between this island and the main land was called Investigator's Strait. Spencer's Gulf is separated from another gulf by a peninsula called Yorke peninsula, about 30 miles broad. This new gulf received the name of St. Vincent's Gulf. It is shorter than Spencer's Gulf, not reaching quite so far north as latitude 34°.

After leaving this gulf Captain Flinders met with Captain Baudin in the French ship Geographe, who was likewise out on a voyage of discovery. He had sailed through Bass's Straits, and had met with foul weather, during which he separated from his consort. Both here and afterwards in Botany Bay, the French captain and his officers admitted that Captain Flinders had been the discoverer of Spencer's and St. Vincent's Gulfs, Kangaroo Island, &c. But when an account of the French voyage was afterwards published in Paris by M. Peron, all this coast was claimed as the exclusive discovery of the French Captain, and named Terre Bonaparte, Golfe Napoleon, Golfe Josephine, &c. This we have no doubt was by the express orders of the then existing French government; was of a piece with that systematic course of fraud and falsehood which Bonaparte uniformly followed during the whole time of his administration. We have no doubt that the French nation will now give up these unwarrantable and absurd claims; and that in the account of Captain Baudin's voyage, just published at Paris, a true statement of the real discoveries of the French captain will be given, while those that really belong to Captain Flinders will be assigned to their true author.

After parting with Captain Baudin, the Investigator sailed nearly in a south-easterly direction, along about 50 leagues of coast which had been examined by Captain Baudin. It was a low, barren, sandy shore, without a single river, opening, or landing place of any kind. It terminated at Cape Buffon, in south latitude about 37° 40′, and east longitude about 140° 5′.

The next portion of coast to Bass's Straits had been explored by Captain James Grant, in the Lady Nelson, in 1800. Except Port Philip, a very large bay about 36 miles long and 30 broad, with a narrow opening of not more than two miles wide; and Western Port beside it, which had been examined by Mr. Bass in his whale boat expedition, the remainder of the south coast presented nothing remarkable. Captain Flinders now made the best of his way to Sidney Cove. Here he remained twelve weeks to refit and provision the ship. It was then agreed between him and Governor King, that he should sail north and examine some part of the east coast of New Holland, Torres Straits, and the Gulf of Carpentaria.

He sailed from Port Jackson on the 22d of July, 1802, with the Brig Lady Nelson, commanded by Lieutenant Murray, in company. As he had already surveyed a portion of this coast in the Norfolk, his examination of it did not properly begin till he had passed Hervey's Bay, in latitude 25°, south. In latitude 23° 50′, longitude east 151° 20′, he discovered a harbour behind an island, called Facing Island, to which he gave the name of Port Curtis. It communicates by a narrow shallow channel with Port Keppel on the north side, discovered by Captain Cook; thus cutting off a considerable portion of land as an island. The country round Port Keppel was the best they had seen since they left Port Jackson. The natives were stout, healthy, and apparently very good natured; for they conducted a master's mate and seaman, who had put themselves entirely in their power, in safety back to the ship.

In latitude 22·5°, a new port was discovered and examined, to which Captain Flinders gave the name of Port Bower. Shoal Water Bay, on the north of this port, but running as far south, and separated from it by a narrow strait, was next examined, and an accurate chart of it made out. This place affords but few temptations for a colony. The land is barren and the water shallow. Pines are found here as well as at Port Bower; but in the latter place they are much more easily got at.

The next object of examination was Broad Sound, which lies a little to the north-west of Shoalwater Bay. Here the tides rise to the height of above 30 feet. The best anchoring place is Upper Head, where a ship may lie secure against all winds; and this is almost the only place in the whole sound where a landing may be easily effected. The country round the sound is tolerably fertile, and well wooded. Captain Flinders thinks it might be taken advantage of for ship-building.

Our navigator now directed his course for Torres's Straits; but was entangled for 14 days among the extraordinary coral reefs, which extend for 14 degrees of latitude along the east side of New Holland, from south latitude 22° 50′ to Torres's Straits. At first they are at a considerable distance from the land; but they gradually approach it as we go north, and a little beyond Cape Tribulation they touch it. Captain Flinders made his way at last through an opening in about latitude 18⅛°, south, and met with no further interruption on his way to Torres's Straits. Through these straits, previously considered as so formidable, he passed without any accident, in three days, by keeping more to the south than former navigators: and there is every reason to consider them as affording the shortest, and a sufficiently safe passage to the southern ocean.

The survey of the Gulf of Carpentaria occupied Captain Flinders 105 days. Its general form is pretty similar to that represented in the old Dutch charts. It extends in longitude from Endeavour's Straits to Cape Wilberforce 5⅛°, and in latitude 7°. The east side of it consists of very low flat land without a single opening; but at the southern extremity, and along the west coast, there are various groups of islands, which were carefully surveyed by our navigator, and laid down in his chart. While in this gulf the Investigator was examined, and found so much decayed as not to be reckoned capable of keeping the sea for more than six months. Of these, three were spent in the Gulf of Carpentaria. Captain Flinders was therefore under the necessity of terminating his survey of New Holland here. He sailed to Copang Bay, a Dutch colony in the Island of Timor, in order to obtain a supply of provisions. From this place he sailed round New Holland by the west and south coasts, and arrived in safety at Port Jackson. Here the Investigator was condemned as utterly incapable of repair, and Captain Flinders resolved to return to Britain for another ship, to enable him to continue his survey, and to leave the scientific gentlemen at Port Jackson till his return. The rest of the voyage is little else than a history of disasters.

The command of the Porpoise, his Majesty's armed vessel at that time lying in Port Jackson, was given to Lieutenant Fowler, first of the Investigator, with a crew of 38 men; and Captain Flinders, with such of his officers as chose it, went on board her to be conveyed to England as passengers. Two vessels lying at that time in Port Jackson, the Bridgewater, Extra East Indiaman, commanded by Captain Palmer, and the Cato of London, commanded by Mr. John Park, desired to accompany the Porpoise and were permitted. These vessels sailed from Port Jackson on the 10th of August, 1803, and on the 18th, in the evening, the Porpoise suddenly struck upon a reef and fell over. The Cato and Bridgewater were coming up in such a direction that in a few moments they must have struck against each other, and been both sent to the bottom. The Cato, to prevent this, generously allowed herself to fall upon the reef. The Bridgewater thus escaped her by a few feet, and got safe out of the reach of the reef. The night was dark. It was uncertain how long the vessels would hold together, and the only chance of safety seemed to be in holding out till morning, when it was fully expected that the Bridgewater would come and take the crews aboard. The cutter was suddenly let down from the Porpoise; but it was supposed to be broken by the violence of the surf. Another boat was let down, and Captain Flinders swam to it, in order to get on board the Bridgewater, and inform Captain Palmer of the situation of the vessels; but the Bridgewater was sailing away, and Captain Flinders found that, with the wind against him, and only two bad oars, it would be impossible to make his way to that vessel. He determined, therefore, to remain near the Porpoise till morning. He found the cutter safe and full of men, without any officers. He told them to remain near him till day-light. Fortunately the Porpoise had fallen over towards the sand bank, so that the sea did not beat over her, and she remained together without being stove in till morning. The case was different with the Cato. She had fallen towards the deep water. Her upper works were immediately beaten in and destroyed, and every thing except the men washed overboard.

Next morning a dry sand bank with birds' eggs upon it, indicating that it was never covered by the tide, was perceived at no great distance from the Porpoise. Here they agreed to land, and to take on shore as many stores as possible from the Porpoise. The Cato's crew made their way by swimming to the Porpoise, and only three lads were drowned; but most of them were in their shirts, and they had to be supplied with clothes by the officers and men of the Porpoise. On this sand-bank they all landed. Captain Flinders assumed the command as senior officer. Almost the whole of the provisions and stores in the Porpoise were landed, and the whole party brought under the same order and discipline as on board his Majesty's ships. The Bridgewater sailed away, without any attempt to relieve the shipwrecked vessels, or to ascertain whether any of the crew were left alive or not: and Captain Palmer, on his arrival in India, gave information that the two vessels were wrecked, and that all on board had perished. The Bridgewater was herself lost on her homeward voyage to Britain, and Captain Palmer never more heard of. This was, perhaps, fortunate for himself; for his situation, after the true history of the shipwreck was known, would have been most uncomfortable, supposing him possessed of any feeling.

Wreck Reef, as the place of the shipwreck was called, lies in latitude 22° 11′ 23″, south; and in longitude 155° 18′ 50·5″, east. It is distant from Port Jackson about 750 miles. Finding themselves abandoned by the Bridgewater, it was agreed in a consultation of officers, that one of them should endeavour to make his way in one of the boats to Port Jackson, in order to procure the means necessary to convey them to a place of safety. Captain

Flinders was selected for this arduous undertaking, and he readily undertook it. Mr. Park of the Cato went with him as second in command. They had a crew of 14 men, and provisions for three weeks; so that they were rather too deeply loaded. However, they made their way without any accident, first to the coast of New Holland, and then along that coast to Port Jackson, in 13 days.

Governor King sent the ship Rolla, and two schooners, to bring away the unfortunate men of the wreck. One of the schooners was to bring back such as chose it to Port Jackson; the other, of 27 tons burthen, under the command of Captain Flinders, with a crew of ten men, was to proceed to England; while the Rolla was to carry the rest of the officers and men to China. Captain Flinders reached Wreck Reef six weeks after he had left it; the stores and provisions of the Porpoise were put aboard the Rolla or the Port Jackson schooner; the men were all embarked to their various destinations; and Captain Flinders set out on his extraordinary voyage to England.

Sailing through Torres's Straits he arrived at Coepang in Timor, only four days after Captain Palmer, in the Bridgewater, had reached Batavia; thus demonstrating the great advantage of sailing through that strait, when compared with the round about way usually followed. From Timor he was obliged to make his way to the Mauritius, because his little vessel could not venture round the Cape of Good Hope without being repaired. He trusted to his French pass, that even if the war should have recommenced, he would be treated in a friendly manner in that island, and be allowed to continue his voyage to Britain. The Governor of the Mauritius was General De Caen, who had been sent out to Pondicherry at the peace of 1801; and, no doubt, expected to make a great figure in India; but the breaking out of the new war in 1802 disappointed his expectations, by depriving the French of all their possessions in India. The Geographe, a French vessel, out on a voyage of discovery, with an English pass, had left the Mauritius on the very day that Captain Flinders reached it; and, contrary to the stipulations contracted by obtaining the English pass, had carried dispatches from the Mauritius to France. This induced General De Caen to detain Captain Flinders for some days. At first he pretended that he was an impostor, and treated him with haughtiness and vulgar insolence; but finding that the plea of impostor could not be continued, he next day, without making any previous apology for his former conduct, invited Captain Flinders to dinner. This invitation Captain Flinders declined, in consequence of the Governor's previous treatment of him. De Caen, it would seem, was offended at this display of spirit; and Captain Flinders's letters to the Governor, though very naturally drawn from him by the situation in which he was placed, contributed considerably to heighten the Governor's resentment. Captain Flinders, at the time, does not seem to have been fully aware of the character of

Bonaparte, or the maxims of his government: otherwise, he would not have expected any redress on account of the justice of his cause, or any shame in his adversaries, on account of the cruelty, injustice, or meanness of their conduct. The Cumberland was seized, Captain Flinders and Mr. Aken committed close prisoners in a tavern; and the crew confined in the common prison. He was afterwards sent to the Garden prison, a large house about a mile out of town, attached to which was a piece of ground for walking. Here he remained 20 months. He was then permitted on his parole to reside in Wilhem's plain, in the highest part of the island, with a range of two leagues all round. All the exertions that were made by the Governor General of India, Lord Bentinck, the naval commanders, the men of science in the Mauritius, the President of the Royal Society, were in vain. Even an order from Bonaparte did not produce the desired effect: De Caen probably assigning as a reason that Captain Flinders was too well acquainted with the island to be permitted to depart. At last, in 1810, when an immediate attack upon the island was expected, De Caen suddenly allowed Captain Flinders to depart, without any new order from France; and even to go to the place from which the attack was expected. Captain Flinders conceives that De Caen wished to get back to France, to share in the glory of his former companions in arms; and that he set him at liberty, because he thought he might contribute materially to the capture of the island.

ARTICLE XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday the 26th of January, a paper by Mr. Hooke was read, On the Construction of Naval Charts. Ships are navigated within sight of land by means of naval charts; in the open sea by means of charts of a particular construction, and by observations of the heavenly bodies. The paper was confined to the first of these. Naval charts at present cannot be used upon deck, without the risk of being destroyed. Mr. Hooke proposes to engrave them on stiff paper, and to varnish them well. He shows how the different problems requisite during a navigation along a coast may be solved; but these solutions could not be sufficiently understood from hearing the paper read, to give any account of them here.

On Thursday the 2d of February, part of a paper by Dr. Watson Philips was read, On the Cause of the Motion of the Heart of Animals. Various opinions on this subject have been entertained by physiologists. Of late, M. Gallois has endeavoured to show that this motion depends entirely upon the spinal marrow, and immediately ceases, when the spinal marrow is removed or destroyed. The motion of the heart, which continues for some time after it is removed from the body, he considers as similar to the motion of any other muscular body from stimuli; and the stimulus in this case is the arterial blood. Dr. Philips related a great number of experiments on rabbits and frogs, which appeared quite inconsistent with M. Gallois's hypothesis. Rabbits were rendered insensible by a blow on the occiput; the spinal marrow and brain were then removed, and the respiration kept up by artificial means. The circulation and motion of the heart continued as usual. When stimuli, as spirit of wine or opium, were applied to the spinal marrow or brain, the rate of the circulation was accelerated. When the hind legs of a frog are kept for two minutes in alcohol, the animal loses the power of motion. In this case it expresses great pain; but if tincture of opium be used instead of simple alcohol, little or no pain is expressed.

On Thursday the 9th of February, Dr. Philips's paper was concluded. He found that the peristaltic motion of the intestines continued after the brain and spinal marrow were removed. When the brain or spinal marrow were suddenly crushed, the effect upon the motion of the heart was much greater than when these organs were removed by cutting, or gradually destroyed by means of a wire. From these experiments Dr. Philips concludes that the action of the heart is independent of the brain and spinal marrow; but that it is capable of being affected by these organs. He conceives, with Haller, that the heart and muscles possess excitability independent of the brain and spinal marrow; that the difference between the voluntary and involuntary muscles depends upon the stimuli; and that animals possess three different sets of organs, the muscular, the nervous, and the sensorial, independent of each other, but capable of influencing each other.

On Thusday the 16th of February, a paper was read from Mr. Clift, describing experiments to ascertain the influence of the spinal marrow on the action of the heart in fishes.

At the same meeting a letter from Dr. Brewster was read, describing a new property possessed by the second surface of transparent bodies.

LINNÆAN SOCIETY.

On Tuesday the 7th of February, specimens exactly similar to Bovey coal, but brought from Constantinople, were exhibited to the Society by Mr. Sowerby. A communication by Mr. Sowerby was also read, On a Species of Fossil Terrebratula.

At the same meeting, two communications from Dr. Mitchell of New York were read. The first gave an account of a singular species of pleuronectes found in the New York rivers. Dr. Mitchell is inclined to consider it as a variety of the white-bellied pleuronectes; though the differences are considerable. Among others, the belly is nearly as dark as the back. The second paper contained a description of a number of species of fish observed by Dr. Mitchell, and which are caught on the coast or in the rivers.

On Tuesday the 16th of February, a letter was read from Sir J. E. Smith to Mr. Macleugh, in which he shews, from a manuscript of the late Dr. Sibthorpe, that the lignum rhodium of Pococke, though not that of the ancients, is the liquidamber styraciflua.

At the same meeting, the conclusion was read of M. Vieillot's Novi Systematis Ornithologici Prolusio.

GEOLOGICAL SOCIETY.

Dec. 16, 1814.—Specimens from Maestricht from the Rev. E. Honey were presented, and a notice relative to the mountain of St. Pierre, near Maestricht, by the same, was read, and the thanks of the Society were voted for the same.

The mountain of St. Pierre is a hill about 150 feet high, which commences within a mile of Maestricht, and extends about three leagues in the direction of Liege. On the side next to the Meuse, it forms nearly vertical cliffs, and thus affords excellent sections of the strata, which are almost horizontal, having only a very slight dip to the north.

The lower beds are decidedly chalk; alternating at every two or three feet, with beds of flint nodules: the fossils of this chalk, though less abundant, appear to exhibit the same species as those which occur in the chalk of England. Above these are beds resembling the former in colour, but harder, and gritty to the touch.

Upon these lie a series of beds of calcareous free-stone, of which the mass of the hill is composed, and in which the extensive subterranean quarries are situated. This stone, in the quarry, is yellowish, and so soft as to be readily cut with a knife; but, by exposure to the air, it becomes both whiter and harder. Interposed between these beds are thin ones, composed chiefly of fragments of madrepores and shells. Beds of flint also occur here, as in the chalk; but the distance between each bed gradually increases, so that those at the top of the series are separated by an interval of eight or ten feet. The fossils of the free-stone are very numerous; the most common are madreporites, fungites, belemnites, nummulites, echinites, ostreites, and pectinites.

The top of the hill is covered by a bed of gravel, in some places of considerable thickness; containing rolled pebbles of flint, of quartz, of grey-wacke; with veins of quartz, and of red sandstone.

The whole of this series of beds, with the exception of the gravel, is considered by Mr. Honey to belong to the chalk formation.

The reading of Dr. Macculloch's paper on Glen tilt was begun.

Jan. 6, 1815.—The reading of Dr. Macculloch's paper on Glen tilt was continued.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Since our last report of this Society, we have to communicate that his Royal Highness the Prince Regent has become its Patron; and, as this is the first instance in Great Britain in which the attention of the throne has been directed to the promotion of mineralogical science, it is to be fairly anticipated that it may lead our government to the establishment of a School of Mines; the great advantage of such institutions having been strikingly evinced in other nations.

It was resolved that a deputation should present an address of thanks to his Royal Highness, for the great honour conferred upon them; and that it should consist of the Vice Patrons, Lord De Dunstanville, and the Earl of Yarmouth; the President, Davies Giddy, Esq. M. P.; and its founder, Dr. Ayrton Paris.

Apartments have been provided at Penzance, which contain a collection of minerals already highly interesting: among the later additions we may notice *rutilute,* lately dicovered in the slate quarries at Tintagel; *a grey copper ore* from Crennis mine, the composition of which resembles the *fal-erz,* with the exception of lead, (on the authority of the Rev. William Gregor.) *Wood tin* from Trethurgy Moor, near St. Austel, in a matrix of shorl and quartz. *The triple sulphuret of antimony, lead, and copper,* which has reappeared at the Antimony Mine, near Port Isaac, after a lapse of twenty years.—*Sulphate of barytes,* now found at Huel Unity, for the first time in Cornwall.—A large quantity of *stream gold,* presented by Sir Christopher Hawkins, with an interesting account of its discovery, in which he states that it was found in streaming for tin in a moor in the parish of Ladock; and offers some information, which he trusts may direct future adventurers to a successful undertaking.—Many other communications have been also read before the Society, an analysis of which we shall offer to our readers in the next number of the *Annals.*—We understand that the Society are preparing a volume of Transactions, which are shortly to appear.

Article XIII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. T. J. Pettigrew, F.L.S. will commence his Spring Course of Lectures on Anatomy and Physiology on Friday, the 10th of March, at half-past eight o'clock in the evening precisely, at his house, No. 8, Bolt-court, Fleet-street, where particulars may be obtained.

Dr. Clarke and Mr. Clarke will commence their next Course of Lectures on Midwifery and the Diseases of Women and Children on Monday, March 20. The Lectures are read at Mr. Clarke's house, 10, Saville-row, Burlington Gardens, every morning from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals.

II. *French Agriculture.*

The following account is given of the present agriculture of France, by Mr. Morris Birkbeck, in his Notes on a Journey through France, in July, August, and September, 1814, page 109.

"In the agriculture of France there is a great sameness. The arable land, which comprises almost the whole surface of the country, the vineyards and a few tracts of mountains excepted, may be divided into five classes, according to its fertility, without regard to the nature of the soil. The first bears a crop every year, as in Auvergne, in the neighbourhood of Thoulouse, in some parts of Normandy, &c. This description is highly cultivated, and on a principle well adapted to soil and circumstances. The second somewhat inferior in quality, but good land, is also judiciouly cultivated, with the intervention of a fallow once in six years, as about Dieppe and Rouen. The third land of middling quality, which embraces a very large part of the kingdom, is managed on the old plan of fallow, wheat, oats. The fourth, poor land, which also covers a large space, is fallow and wheat alternately. The fifth, poor land, is cultivated in the round of fallow, rye, rest, without grass seeds. The first and second classes include what there is of variety and spirit in French husbandry. In the south, Indian corn alternating with wheat, exhibits management as good as the beans and wheat of the best English farmer: and the varied outline observable in the north, affords many proofs of a spirited and judicious culture. It is the three last which betray its weakness; if they comprise half the cultivated surface, which I believe is not over-rating their extent, half of that portion being fallow, it appears that one fourth of the whole country is lying in a state entirely unproductive; a few weeds, mostly thistles, excepted. A very few half starved sheep are kept to pick over the constantly recurring barren fallows, often accompanied by three or four long legged hogs. On the borders, and out of the way corners, you may see a cow or two, with an attendant; but there appears so little for any of these animals to eat, that you wonder how even they are supported. The prairies artificielles, (the artificial grasses, as we less properly call them,) of which so much is said by the amateurs, are like specks of green on a desert. Clover and lucern are cultivated with great success, on the two first classes of land; but very rarely indeed on the others. Thus there is probably as much really waste land in France as in England, and it is of an expensive kind; whereas our wastes support much more stock than theirs, without any expense whatever."

By Mr. Birkbeck's account, the labouring classes in the country parts of France are in much better circumstances, and receive much better wages than the labouring people in England. Most of them are proprietors of eight or ten acres of land, having been enabled to make the purchace during the revolution. He considers their situation, their information and even their morals, as greatly improved since the revolution. Prices in France are about one half of what they are in England.

III. *Dr. Cross's Opinions respecting the Cerebellum, &c.*

(To Dr. Thomson.)

MY DEAR SIR,

In the last number of your *Annals* I observed a paper On the Use of the Cerebellum, &c. containing some supposed new observations on the structure of the spinal mass of nerves, and on the function of its parts. Although I have no wish to detract from the originality of Dr. Cross's statements, yet I feel myself bound in justice to inform you that the same facts, or facts that lead to similar conclusions, are published in the following works:— Lettres de Hufeland a Portal, 1807. Anatomie du Systeme Nerveux en general, &c. par Gall et Spurzheim.

By inserting a notice of the contents of this note, you will much oblige, your obedient servant,

British Museum, Feb. 3, 1815. W. E. LEACH.

IV. *On the Petrifactions in Plymouth Lime-stone.*

(To Dr. Thomson.)

MY DEAR SIR,

In the last number of your Journal, p. 150, Mr. Hannah is said to have discovered madrepores and shells in the lime-stone of Plymouth. I am induced, for two reasons, to trouble you with a very few lines on the subject. In the first place, madreporites were first observed in the Plymouth lime-stone about eight years ago (and a notice of this circumstance is given in vol. ii. p. 465, of *Annals of Philosophy*); but it is to Dr. Lockyer, of Plymouth, that we are indebted for the discovery of the exact bed in the lime-stone of that district in which the madreporites are found. The same Gentleman (Dr. Lockyer) also first observed turbinated univalves in loose blocks of lime-stone imbedded in earth on Stonehouse Hill, but shells have no where been observed in the lime-stone of Plymouth in situ. In the second place, the Rev. Mr. Hannah some time since showed me a specimen of lime-stone of a fine blood-red colour full of madreporites: at the same time he informed me that he found it at a place called the Devil's Point, Stonehouse; and as I had never observed this variety of rock near Plymouth, I lost no time in examining the point on the same day; and as the tide was out, and the place not very extensive, I may with confidence assert that no bed or vein of lime-stone similar to that shown me by Mr. Hannah occurs at or near the Devil's Point; but as vessels often throw out ballast near that spot, I have no doubt that the fragments he found came there in that manner.

I should feel it necessary to apologize for troubling you, but for the statement respecting the discovery of the univalve in situ, which will I trust afford a sufficient excuse.

I am, dear Sir, your obedient servant,

British Museum, Feb. 3, 1815. W. E. LEACH.

V. *The Caucasus.*

Engelhart and Parrot, during their travels in the Caucasus, ascertained that some of the peaks of that vast Alpine country are equal in height to Mont Blanc, in Switzerland.

VI. *Heliotrope.*

Dr. Macculloch, of Woolwich, has discovered heliotrope in the Hill of Kinnoul, near Perth. This is the second time that this rare mineral has been observed in Scotland.

VII. *Quantity of Paper at present made in the United States of America.*

	Tons.	Reams.	
For Newspapers*	500	50,000	$150,000
Books	680	70,000	245,000
Writing	650	111,000	333,000
Wrapping	800	100,000	83,000

VIII. *Burning Gas in the Appennines.*

(To Dr. Thomson.)

SIR,

In a preceding number of your Journal you have given an account of a "jet of burning gas," which you say was *discovered* by Sir H. Davy in the Appennines, and found by him to consist of carbureted hydrogen. In addition to this, you make some inquiries respecting the nature of the country, and the probability of the existence of coal among the Appennines.

Knowing the fact to have been confidentially communicated in a private letter written by Sir H. to this country, I confess I felt some surprise at seeing it appear in a public journal; nor was the defective manner in which the statement was made in your *Annals of Philosophy* calculated to diminish that ungrateful sensation.

The mentioning of this gas occurs in Sir H. Davy's letter as one of the many philosophical *observations* which that indefatigable inquirer has been constantly making during his travels through Italy, and from which the world will, I am confident, derive hereafter, whenever he shall think it proper *to make them public*, very great and useful information. As to the word *discovery*, it never once occurs in speaking of this subject; for Sir H. was perfectly aware that the phenomenon, known to all classes of persons travelling on the road from Florence to Bologna, had been particularly noticed by Spallanzani, who had perceived the smell of hydrogen in the gas; then by your own countryman, Faber, in 1772; and since by Lalande and others. Nor has Sir Humphrey in his letter ever mentioned the height of the column of the gas, as stated by you, since this is greater or less according to the state of the atmosphere, which exerts also much influence on its combustion, it being more vivid at the approach of rainy weather.

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* The number of newspapers printed annually in the United States is estimated at twenty-two and an half millions.

But you were quoting from memory, and under circumstances unfavourable for publication. You may, therefore, probably, be anxious for some corrections which my knowledge of the country enables me to submit to your judgment.

The jet of gas alluded to is seen on the top of Monte di Fo, half a mile on the right of Pietramala, coming from Florence. The column occupies in circumference a surface of from ten to twelve feet. The ground around it is covered with large and small masses of primitive rocks, or rather of fragments of quartz and *mica argentine* held together by an argillaceous cement, and of lime-stone. Very feeble signs of vegetation are perceptible, either on this or the surrounding mountains. Monte Fo forms part of Mount Radicoso, or of the highest of the Appennine Ridge, being 883 metres, or 2901 English feet, above the level of the Mediterranéan sea. About half a league from this gas-volcano, and from *Pietramala*, there is a well of cold water, called *Acqua buja*, from which there is a constant evolution of what I myself found to be carbureted hydrogen, which takes fire at the approach of a lighted taper, and burns with a brilliant yellow flame. The Italian naturalists had hitherto considered these emissions of subterraneous gas, by them called *fumarole*, to be *sulphureted hydrogen*, the formation of which they derived from a decomposition of pyrites, that are found in great quantities at a considerable depth in the Appennine country. They fancied they discovered the sulphur, which the decomposition of the gas set at liberty, incrustating the objects surrounding the spot from whence arose the gas; and I confess having myself been of the same opinion during my last visit to the Appennines in the course of last summer. My examination, however, was very superficial, and too hasty to be correct. That of Sir Humphrey Davy is, of course, of quite a different character.

The Appennines consist chiefly of secondary rocks, amongst which lime-stone containing remains of marine animals is predominant. As they, however, approach the Alps, whence they took their origin, they participate more and more of their nature, and present for a long successive tract of ground a mixture of primitive rocks of various species, such as serpentine, argillaceous schist, grunstein, and sacharoid lime-stone. As to the existence of coals in the Appennines, Soldani, in 1780, in his Ornithographic Essay on the Nautilific Formations of Tuscany, mentions a considerable stratum of coal being discovered near Fiesole, a place at the distance of about 40 miles due south from *Pietramala*. Subsequent examinations, however, instituted for the purpose, and directed by very able mineralogists, have not been followed by any result, from which one might be led to conjecture the presence of coal in any part of the Appennines, except some traces of it, which the lowest parts of the sub-appenine country offer, though of very little moment.

I have the honour to be,
Sir, your humble servant,

Brompton, Jan. 22, 1815. A. B. GRANVILLE, M. D.

IX. *Answer to the Queries respecting Shell-Fish in a Moss near Elgin.*

(To Dr. Thomson.)

SIR,

On perusing the last number of your *Annals*, I observed a note signed C. T. requesting information respecting live shell-fish which are said to be found at the depth of three or four feet in the solid body of a moss near Elgin, in Murrayshire.

Though extremely sceptical of the existence of this circumstance, I addressed a letter to the Rev. Wm. Leslie, Minister of St. Andrews, near Elgin, a Gentleman well known for the accuracy of his observation, and zeal in investigating whatever is interesting, requesting he would inform me if there was any foundation for such a report. Mr. L. has kindly favoured me with an account of the different mosses in the vicinity of Elgin, with whatever appeared worthy of notice respecting them. He assures me there is not the least measure of reality, or any kind of foundation for the report alluded to, as no trace of shell-fish, either dead or alive, were found in any of the mosses near Elgin, nor to his knowledge in any other mosses in the adjacent country.

I was some time ago informed by Mr. Hughes, the Gentleman who superintended the operations in draining the lake of Spynie, in the vicinity of Elgin, that a few live fresh-water muscles, and some belemnites, were found in the bed of the lake. The existence of live shell-fish and petrified tangles (as the belemnites were termed) in fresh water was considered by many who saw them as a very extraordinary circumstance; and it is not unlikely but a continuing increase of exaggeration in the detail of this circumstance may have sunk the live shell-fish three or four feet into the solid bed of the lake.

I am, Sir, your most obedient servant,

Inverness, Jan. 21, 1815. J. J. NICOL.

X. *Notice of a remarkable Meteor which appeared on the 2d of December*, 1814.

(To Luke Howard, Esq.)

SIR, *George Yard, Lombard-street, Dec.* 14, 1814.

At your request I now give you the best account I am able to afford of the late luminous meteor.

On Friday night, the 2d of Dec. at about 20 min. before 11, I was walking in an open part of the village of Peckham, about four miles S.S.E. of London. The night was cloudy and dark, the lower part of the atmosphere clear and calm, a *very slight* wind blowing from the E. Suddenly I was surrounded by a great light. I remember that at the instant I shrunk downward and stooped forward; as I was apprehensive of some danger behind me, I instantly ran a few paces. I turned about in a few seconds to the N.E.; for I was certain the light came from that part of the heavens (as it brilliantly illuminated some houses to the S.W. of me); and I think at a considerable height from the horizon. But I saw nothing to cause this light. It did not give me the idea of the force and intensity of lightning; its brilliancy was not so instantaneous and fierce; but it was a softer and paler kind of light, and lasted perhaps three seconds. I could discover no noise, though immediately I expected an explosion.

The strength of the light was nearly equal to that of common day-light; all near objects were distinctly visible. The light very much resembled that of the luminous balls thrown from a sky-rocket when it finally explodes. It was not as vivid and blue as that of a strong flash of lightning at night. None of the persons I met that night thought it to be lightning, though none of them saw any thing but the light. Unless it had been very high, if it was in the direction I have supposed, it would be concealed from them by a high wall and some houses.

A relation of mine, who resides at the northern extremity of Tottenham, saw the light of it as he lay in bed, through a window facing the west. He describes it to have been as light as day.

I am, Sir, respectfully yours,
John Wallis.

XI. *Weather at Calcutta.*

During the winter months at Calcutta there is always a fog every evening and morning. In the evening it rises high enough to cover the ground floors of the houses; but the upper stories are still visible. On this account the ground floors are considered as unhealthy, and are never inhabited by Europeans; but employed either as warehouses, or as lodgings for black servants. In the morning the fog is still more intense, and rises higher, so as to conceal the sun from view an hour and a half after his rising. When the sun sets through the fog he assumes a beautiful vermilion colour. The fog itself acquires the same tint. It is reflected from the water of the river; so that all nature appears painted of the finest red imaginable.

XII. *On the Mode of conveying Gas for Lighting the Streets.*

(To Dr. Thomson.)

SIR,

In answer to your Correspondent respecting the manner in which the Gas Light Companies force the gas through the pipes, I beg to inform him that, from its very compressible nature, and the number of collateral branches from the main to the lamps, forming so many outlets, that it is sent through the pipes with a very small pressure: I believe of not more than equal to a column of water of one inch in height. Should you consider the above worth inserting, I shall send you a paper on the structure of the cells of wasps and bees, which I consider so different as to require a particular explanation.

I remain, Sir, yours truly,

R. W.

XIII. *Mr. Accum's Treatise on Gas Light.*

Mr. Accum has in the press a Treatise on Gas Light, exhibiting a summary description of the apparatus and machinery best calculated for illuminating houses, streets, and public edifices, with carbureted hydrogen, or coal gas; together with remarks on the utility, safety, and general nature of this new branch of civil economy.

The treatise will be illustrated with geometrical and perspective designs, exhibiting the larger gas light apparatus now successfully employed for lighting the streets and houses of this metropolis, as well as the smaller apparatus used by certain manufacturers and private individuals, together with other objects connected with this new art of illumination.

XIV. *Metallization of Charcoal.*

Mr. Dobereiner, Professor of Chemistry at the University of Jena, in Saxony, informs Mr. Accum that he has discovered charcoal to be a metallic substance. The following statement has been received from Mr. Dobereiner by Mr. Accum:—

"I hope soon to be able to communicate to you the successful metallization of charcoal, which I have reason to believe to be a metallic substance; in cast-iron and in steel the metal which it contains is present in a metallic state, and may be separated from both of them by the united action of phosphorus and an alkali."

Farther particulars concerning this subject Professor Dobereiner promises to communicate in his next.

XV. *On the Octohedral Form of Iodine.*

(To Dr. Thomson.)

DEAR SIR,

I regret that I was not aware of your intention to notice my observations on the crystals of iodine, as I would have undertaken to define with more precision the primary form from which all its other modifications may be readily inferred.

You have stated, (*Annals* for Jan. p. 12,) that it assumes an octohedral form, which, though true, is liable to be misinterpreted; since the crystal may be supposed equilateral and equiangular, unless its peculiar deviation from the regular octohedron be specified.

The axes of the octohedral crystal of iodine instead of being equal, are to each other in the proportion of the numbers 2, 3, and 4, so nearly that, in a body so volatile, it is scarcely possible to detect an error in this estimate by the reflective goniometer, because the surfaces change by evaporation too rapidly for any precise measurement of their mutual inclination.

The form which most frequently appears in a section of this octohedron parallel to the plane of its greatest and least axes, presenting to view a rhombic plate bevilled at each of its edges by two narrow planes, which are inclined to each other at an angle of about 120¼°.

From the frequent occurrence of this rhombic plate, (of which the acute angle is about 53°,) some crystallographers may be disposed to regard this surface as the terminal face of a rhombic prism, of which the diagonals are 2 and 4, and its height 3; and it is evident that all the modifications of the octohedron may with equal facility be derived from this prism.

I remain, Dear Sir, with great regard,
Your obliged and obedient Servant,
Feb. 22, 1815. Wm. H. Wollaston.

P. S. In the same Number of your *Annals*, p. 75, you have noticed the discovery of chromate of iron in America, crystallized in regular octohedrons found in serpentine.

Permit me to add, that this rare mineral, which I believe has not hitherto been observed in Britain, occurs also in the Serpentine of Portsoy, having the same crystalline form, and tinging the adjacent rock of the usual green colour of chrome.

Article XIV.

New Patents.

Larger Didot, Paddington, Middlesex; for certain improvements in the method or means of illuminating houses or places, by the combination of tallow or other inflammable materials. Nov. 10, 1814.

William Benecke, Deptford, Kent; for the improved method of manufacturing verdigris, of the same quality as that known in commerce by the name of French verdigris. Nov. 12, 1814.

Horace Hall, Golden-square, London; for an improved method of preparing and spinning hemp, flax, and other substances. Communicated by a foreigner residing abroad. Nov. 17, 1814.

Edward Massey, Coventry; for his chronometers and pocket watches. Nov. 17, 1814.

Robert Barlow, Francis-street, Southwark, surgeon; for a machine or instrument called the *hydrostatic self-blowing machine*. Nov. 22, 1814.

ARTICLE XV.

METEOROLOGICAL TABLE.

		BAROMETER.			THERMOMETER.					
1815.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain.	
1st Mo.										
Jan. 2	N E	30·45	30·42	30·435	35	32	33·5			☾
3	N E	30·42	30·16	30·290	33	30	31·5			
4	N	30·16	30·06	30·110	33	30	31·5			
5	N E	30·06	30·05	30·055	31	30	30·5			
6	N	30·05	29·95	30·000	34	25	29·5			
7	N W	29·95	29·48	29·715	35	24	29·5			
8	W	29·99	29·48	29·735	34	25	29·5			
9	Var.	30·02	29·76	29·890	42	26	34·0			
10	N W	29·76	29·52	29·640	44	35	39·5	·13		●
11	N W	29·88	29·52	29·700	40	32	36·0			
12	N W	30·15	29·88	30·015	40	24	32·0			
13	S W	30·15	29·75	29·950	40	28	34·0		·28	
14	N	30·17	29·75	29·960	43	32	37·5			
15	N E	30·30	30·17	30·235	36	27	31·5			
16	Var.	30·25	30·20	30·225	37	30	33·5			
17	N W	30·25	30·13	30·190	41	32	36·5	·19		
18	N	30·13	29·96	30·055	36	27	31·5			☽
19	E	29·96	29·68	29·820	34	22	28·0			
20	N E	29·80	29·77	29·785	28	26	27·0			
21	N E	29·80	29·75	29·775	35	29	32·0			
22	N	29·85	29·80	29·825	35	28	31·5			
23	Var.	29·85	29·73	29·790	37	17	27·0			
24	Var.	29·61	29·56	29·585	29	20	24·5			
25	N	29·56	29·41	29·485	30	22	26·0			○
26	S E	29·41	28·96	29·185	39	23	31·0			
27	E	28·96	28·88	28·920	39	30	34·5			
28	S W	29·03	28·95	28·990	42	36	39·0		·57	
29	Var.	29·24	29·03	29·135	43	34	38·5			
30	S E	29·29	29·22	29·255	41	35	38·0			
31	S	29·36	29·34	29·350	45	38	41·5	·17	·22	
		30·45	28·88	29·770	45	17	32·66	·49	1·07	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

RESULTS.

First Month, 1815. — 2. Slight hoar frost: misty: cloudy. 3. Breeze fresh, and drier air: slightly clouded. 4. The same. 5. The same: a little snow. 6. The same. 7. a.m. A bank of *Cirrostratus* in the S.E.: during the forenoon these clouds passed over in flocks, and becoming denser at night, there fell a little rain or sleet. 8. A fine day, with *Cirrus:* bright star-light night. 9. Clear morning, but a *Cirrostratus* over the marshes, and *Cirrus* above in lines from N.E. to S.W.: at mid-day a little snow: windy evening. 10. *Cirrostratus* and *Cumulostratus: max.* of temp. for the day this morning. 11. Brisk wind: *Nimbi* to the S., succeeded by *Cumulus* and *Cirrus:* a little snow after dark. 13. a.m. Barom. falling: hoar frost: red lowering *Cirrus* and *Cirrostratus* clouds, in lines from N. to S.: these indications were followed by rain after two p.m. 15. Cloudy: smart breeze: S.E. in the night. 16. a.m. The sky overcast with *Cirrostratus:* this modification continued through the day, with a dry air. 17. Cloudy: windy night. 18—23. Snow fell at intervals during these six days, often in regular and beautiful crystallizations. 24. a.m. Much rime on the trees and shrubs: misty from *Cirrostratus:* temp. 18° about nine: wind easterly in the night. 25. Somewhat misty: overcast: snow. 26. Cloudy, a.m.: snow, p.m. in flakes of all sizes, varying from the simple union of six prisms in a minute star to broad feathery flakes of the most regular compound structure: it was nearly calm during this time; so that the crystals escaped the derangement consequent on being driven about in their descent. 27. Snow and sleet, p.m.: a thaw, interrupted by a little frost, in the evening: max. of temp. in the night. 28. Wind and rain in the night. 30. a.m. Misty: the trees dripping: rain.

REMARKS.

Winds Northerly till near the close of the period.

Barometer: Greatest height..........30·45 inches;
Least...................28·88
Mean of the period......29·77

Thermometer: Greatest height..........45°
Least...................17
Mean of the period......32·66

Evaporation, 0·49 inch. Snow and Rain, 1·07 inch.

Tottenham,
Second Month, 24, 1815.

L. HOWARD.

ANNALS

OF

PHILOSOPHY.

APRIL, 1815.

Article I.

A Biographical Account of Sir Benjamin Thompson, Knt. Count Rumford. By Thomas Thomson, M.D. F.R.S.

Sir BENJAMIN THOMPSON was born in the year 1752, in the little town of Rumford, in New England. His parents, who were in middling circumstances, gave him the best education the place could afford. He married, early in life, the school-master's daughter of the place; and I have been told, though I cannot vouch for the accuracy of the information, that he himself for some time discharged the duties of school-master. This is by no means inconsistent with the rank which he held, as Major of the Militia of this district; as in America the military officers did not constitute a separate profession, but were selected out of the most respectable inhabitants of the country, who still retained their old situations, and continued their old pursuits.

When the American revolutionary war commenced, Mr. Thompson embraced the side of the mother country, and was of considerable service, from his local knowledge of the country. He soon came over to London, with proposals to raise a regiment in America for the service of his Majesty; and such was his address and insinuating manners, that he acquired the confidence and friendship of Lord George Germaine, at that time at the head of the Colonial Department, who gave him, it is said, a situation in his office. He was afterwards sent over to New York to raise the proposed regiment, which he accomplished. In consequence of this, when the peace was concluded in 1783, he became entitled to half-pay. In 1784 he was knighted by his present Majesty.

He had been elected a Member of the Royal Society in the year

1779; and he began to distinguish himself as an experimental philosopher in 1781. In 1784 he made a tour on the Continent, and became acquainted with the present King of Bavaria at Strasburgh, who was at that time Prince of Deux Ponts. He insinuated himself so successfully into the good graces of this Prince, that he recommended him in the most powerful manner to his relation and predecessor the then Elector Palatine, who invited him into his service on the most honourable terms. He accepted the offers that were made him, and was employed by the Elector in new modelling the army, and in introducing various changes and reforms into different departments of government. He received a pension from the King of Bavaria amounting to about 900*l.* a-year, which constituted, I believe, almost the whole of his income. I have been told, though I do not believe the statement, that the Elector Palatine had applied to the British Government for a proper person to organize his army, and that Sir Benjamin Thompson was recommended by Ministry for that purpose. Be that as it may, he seems to have enjoyed the full confidence of the Elector Palatine; and he remained in Bavaria till 1799, when he returned to Great Britain; still, however, retaining his Bavarian pension.

During this long interval he had distinguished himself by various papers published in the Philosophical Transactions, on the Force of Gunpowder and on Heat, all of them written in the most pleasing and fascinating style, and containing much curious and important information. On his return to London in 1799 he began to make known those speculations which seem to have occupied a good deal of his attention during his residence in Germany. They regarded chiefly improvements in the construction of our fire-places, and in the mode of preparing food for the poor. His alterations in our fire-places constituting a real improvement; and Rumford grates, as they were called (for by this time Sir Benjamin Thompson had been created a Baron of the German Empire), very soon became almost universal in Great Britain. These improvements gave him a degree of popularity very uncommon among literary men in this country; so that for some time Count Rumford constituted the general subject of conversation.

1799 and 1800 were two years of very bad crops, which succeeded one another; the one from too much rain, the other from too long a continuance of dry weather. It was then that the prices rose to their present pitch; and they have never since fallen to what was formerly considered a reasonable rate. These high prices occasioned subscriptions in different parts for the relief of the poor. The Count's popularity enabled him to suggest a plan which he had put in execution in Germany, and to procure its adoption. Large quantities of soup were made, and distributed either by means of tickets, or sold at a very cheap rate. I had an opportunity of seeing this practised for two successive winters in Edinburgh upon a very considerable scale. But though the plan appeared good in an abstract point of view, it was not found upon trial to answer so well as had been expected. Many of the greatest objects of charity were deterred by a false shame from making application for the soup, because they considered such an application as a declaration of their poverty before the whole town, and as sinking them a step lower in the scale of society. Those who came forward with the greatest effrontery were sufficiently poor indeed; but too frequently their characters were not of the best stamp, and not a few of them by a vicious course of life had lost that regard for character, and that desire of the good opinion of others, which constitute so important a part of the feelings of the common people in those countries where they have not been vitiated by improper institutions and erroneous laws. Though those who had the office of preparing the soup discharged their duty with great fidelity and honour, yet the poor people were not satisfied, but complained loudly against the quality of the soup, and the honesty of the purveyors.

These two years of scarcity seem to have directed the Count's attention to the art of cooking. Combining his notions of economy, and his opinions respecting heat, he contrived a new cooking apparatus by means of steam, which he assured the public was greatly superior in every respect to the old mode. Such was his popularity at that time, that numbers of people adopted his ideas, and fitted up their kitchens according to his models; but I have not heard that his scheme was found to answer in a single instance. I remember going in 1802 to see the Count's own kitchen, which was fitted up according to his own plan, and was at Brompton, or somewhere about Knightsbridge. I was very much surprised to observe that not one of the utensils had ever been put to use. Hence it was likely that his notions of cooking were rather theoretical than practical.

He had begun, soon after his arrival in London in 1799, to publish a series of essays in succession on different subjects; but all connected with his own favourite pursuits. These essays amount in all to 18 or 19, constituting two octavo volumes. They were exceedingly popular at their first appearance, and very generally read; and they contain many valuable facts, put together in a pleasing and entertaining manner.

It was at this time that he had influence enough to procure the formation of the Royal Institution as a school for science in the metropolis of Great Britain, and a place where models of every kind were to be collected and exhibited. This Institution has flourished ever since, and has given birth to various others upon a similar plan, both in London and in other towns of Great Britain and Ireland.

The uncommon popularity which the Count enjoyed for some years seems to have produced a bad effect upon his disposition, or perhaps rather induced him to display without reserve those dispositions which he had hitherto been at some pains to conceal. Pomposity, and a species of literary arrogance quite unsuitable to the nature of experimental philosophy, for some years characterized his writings, and injured their value. But in some of the last essays with which he favoured the world we find much valuable and curious information, respecting the heat evolved by different combustibles while burning, a subject of great interest, which he prosecuted for many years, and at last elucidated with considerable success.

I pass over his quarrel with the managers of the Royal Institution, about the nature of which I am not fully informed, though I suppose it was an attempt on the part of the Count to retain in his own hands the entire management of that Institution. Be that as it may, the result of the dispute induced him to leave London, to which he never again returned. He settled at Paris, and some years after married the widow of M. Lavoisier, who had retained part of her unfortunate husband's property; but their tempers were not found to be congenial. Hence after some time they parted. He lived in a house at Auteuil, in the neighbourhood of Paris; and during the two or three last years of his life, his daughter, who had been brought up in America, but who came over to join him in France, lived with him in the same house. He died on Sunday, August the 21st, 1814, in the 62d year of his age. His writings, so far as I am acquainted with them, are the following:—

1. New Experiments on Gunpowder. Phil. Trans. for 1781. P. 229.—This is an elaborate paper. The experiments appear to have been made with great care. His method was similar to that contrived by Robins, and universally known. I can only give a general idea of some of the points which he established. He found, as Robins had done, that when the powder was rammed into the piece the effect was greater than when it lay loose, and therefore recommends the use of the ramrod in charging a piece. The force of the charge increased as the piece acquired heat by firing. This is so well known in the navy that, after firing two or three times, it is customary to diminish the quantity of powder used. Our author found that the barrel became much hotter when the piece was only charged with powder than when a ball was employed. He conceives the heat to be produced by the vibration of the barrel, and supposes that this vibration will be greatest when there is no ball, because in that case the action on the barrel is only momentary. Here we have the first notice of our author's peculiar notions respecting heat, which he retained during the whole of life. It appears from his experiments that the relative velocities of the bullets, supposing every thing else equal, are in the subduplicate ratio of the weights of the charges nearly. This is conformable to preceding experience. The position of the vent produces very little effect on the charge. He points out a method of proving the relative goodness of powder with great accuracy. But the method had been already introduced by Dr. Hutton, and was in use at Woolwich. It consists in having a standard powder, the velocity with which a particular charge of which drives a bullet of a given

size and weight is known. According as the powder under examination drives the bullet with more or less velocity than this powder, it is above or below the standard. From Robins's theory it follows, that when bullets of the same size, but of different weights, are discharged from the same piece by the same quantity of powder, their velocity is reciprocally as the square roots of their weights. But our author shows that there are circumstances which prevent this theory from holding good in all cases. The explosive force of *aurum fulminans* he found equivalent to about 307 atmospheres, or about $\frac{1}{5}$ of that of gunpowder.* The specific gravity of gunpowder he found 1·745. When well shaken, its weight, compared to the same bulk of water, was 0·937 to 1·000, and when lying loose as 0·836 to 1·000. He attempted to increase the force of gunpowder by mixing it with carbonate of potash, sal-ammoniac, and brass filings; but the effect was the reverse of his expectation. The swimming bladders of small fish filled with water or alcohol, and put into the middle of the charge, likewise diminished the effect very considerably.

2. New Experiments on Heat. Phil. Trans. 1786. P. 273.—A thermometer surrounded with different mediums was plunged into boiling water, or into melting ice, and the times that elapsed during the heating or cooling were considered as indicating the conducting power of the different mediums inversely. The following table exhibits the conducting power of the different mediums tried according to these experiments:—

Mercury	1000
Moist air	330
Water	342
Common air, density 1	80$\frac{2}{5}$
Rarified air, density $\frac{1}{4}$	80$\frac{1}{4}$
Rarified air, density $\frac{1}{24}$	78
The Torricellian vacuum	55

But before the conducting power of these substances can be concluded from these experiments, it would be necessary to take into view their specific heats, and likewise the radiation of heat through different elastic mediums.

3. Experiments on the Production of Dephlogisticated Air from Water with various Substances. Phil. Trans. 1787. P. 84.—Dr. Ingenhousz had discovered that when the leaves of plants are put under water, and exposed to the rays of the sun, a quantity of oxygen gas is evolved; and Dr. Priestley had observed that when water became green it always yielded more of this gas than common water. On these experiments a theory had been founded that vegetables decompose water, retaining the hydrogen, and giving out the oxygen, and that by this process the oxygen taken from common

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* I conceive its force does not exceed $\frac{1}{30}$th of that of gunpowder.—T.

air by animals and by combustion is restored. The present experiments show that raw silk, eider down, and various similar bodies, may be substituted for the leaves of plants without diminishing the evolution of oxygen gas. The light of lamps was found to produce the same effect as solar light; but our author did not obtain gas by heat when light was excluded. He confirmed Dr. Priestley's observation, that green-coloured water produces much oxygen gas, but the green matter, according to him, is not of a vegetable nature, but consists of a congeries of animalcules. This subject of the evolution of oxygen gas by exposing various substances under water to the action of the solar rays is not yet cleared up. When our author wrote, chemical analysis was not far enough advanced to enable experimenters to determine exactly the nature of the gases evolved, and modern chemists have not paid any attention to the subject.

4. Experiments to determine the positive and relative Quantities of Moisture absorbed from the Atmosphere by various Substances under similar Circumstances. Phil. Trans. 1787. P. 240.—The following table exhibits the result of these experiments:—

Substances.	Weight after being dried 24 hours in a hot room.	Weight after being exposed 48 hours in a cold uninhabited room.	Weight after being exposed 72 hours in a damp cellar.
Sheep's wool	1000	1084	1163
Beaver's fur	1000	1072	1125
The fur of a Russian hare	1000	1065	1115
Eider down	1000	1067	1112
Silk { Raw, single thread	1000	1057	1107
Silk { Ravellings of white taffety	1000	1054	1106
Linen { Fine lint	1000	1046	1102
Linen { Ravellings of fine linen	1000	1044	1082
Cotton wool	1000	1043	1089
Silver wire, very fine, gilt, and flatted, being the ravellings of gold lace	1000	1000	1000

5. Experiments on Heat. Phil. Trans. 1792. P. 48.—The object of these experiments is to determine the conducting power of various bodies. He surrounded the bulb of a thermometer with various bodies inclosing the bodies and bulb in a glass ball. The thermometer was raised nearly to a boiling heat, and then plunged into ice-cold water. The time of cooling a certain number of degrees was noted, and the conducting power considered as inversely as this time. The following table shows the principal results:—

Air	576″
Hare's fur	1315
Eider down	1305
Beaver's fur	1296
Raw silk	1284
Sheep's wool	1118
Cotton wool	1046
Fine lint	1032

The finer the fibres were the worse a conductor of heat was the substance. He ascribes this to the attraction between the fine fibres and air, which prevents the air from moving out of its place, and thus carrying off the heat. In these experiments, likewise, the radiating power of different bodies is overlooked.

6. A Method of Measuring the comparative Intensities of the Light emitted by Luminous Bodies. Phil. Trans. 1794. P. 67.—The method is to make the luminous bodies compared shine on a sheet of paper in the middle of which is placed a small cylindrical body. This cylinder will project two shadows, one illuminated by one of the luminous bodies, the other by the other. The distance of one of the lights is to be varied till the two shadows are of equal intensity. The light emitted by each body is as the square of its distance from the cylinder. This apparatus our author called a Photometer. It is nearly similar to the method long before employed by Bouguer, of which Count Rumford was probably not aware, as he takes no notice of it. The facts ascertained by Count Rumford by means of this apparatus were the following:—No perceptible quantity of the light is absorbed during its passage through air. When light passes through fine mirror glass about ⅕th is absorbed. About ⅓ is lost when light is reflected from a good plane glass mirror. A good Argand's lamp gives as much light as eight wax candles ¾ inch in diameter. The quantity of oil consumed in an Argand's lamp to that consumed in a common lamp to produce the same light is 16 per cent. less. The light of a candle fluctuates much more than that of a lamp. A tallow candle newly snuffed gave out a quantity of light = 100; in 29 minutes the light was reduced to 16. On being snuffed, it recovers its original intensity. The following table exhibits the weights of different substances, which must be consumed in order to produce the same quantity of light:—

	Weight.
Bees' wax. A good wax candle kept well snuffed, and burning with a clear bright flame	100
Tallow. A good tallow candle kept well snuffed, and burning with a bright flame	101
The same tallow candle, burning very dim for want of snuffing	229
Olive oil burnt in an Argand's lamp	110

	Weight.
Olive oil burnt in a common lamp, with a clear bright flame without smoke	129
Rape oil. Burnt in the same manner	125
Linseed oil. Likewise burnt in the same manner	120

But it is necessary to observe that these experiments were afterwards repeated by M. Hassenfratz, who obtained a very different result. He found that more oil must be consumed to produce the same quantity of light in an Argand's lamp than in a common lamp.

7. An Account of some Experiments on Coloured Shadows. Phil. Trans. 1794. P. 107.—It is well known that when two shadows are cast upon paper, one illuminated by the light of day or of the moon, the other by the light of a candle, the first appears blue, the second yellow. Count Rumford observed this by accident, and he was induced to make a set of experiments on the subject. He found that the two colours could be produced by two candles by interposing a pane of yellow or blue glass before one of them. When two rays of light from different parts of the heavens on a windy day were made to illuminate two shadows, a most fascinating succession of colours took place, all perfectly harmonious, and exhibiting every possible variety. He found that the blue colour was merely an optical deception, being always produced when the other shadow was yellow. But when he surveyed the blue shadow alone through a long tube which excluded from sight the yellow shadow and all surrounding objects, no blue colour was perceptible.

8. Experiments to determine the Force of fired Gunpowder. Phil. Trans. 1797. P. 222.—This is perhaps the most curious and interesting of all Count Rumford's papers. The experiments are very ingenious, and appear to have been well conducted; though the conclusions which he deduces from them are by no means satisfactory. Robins estimated the force of gunpowder at 1000 atmospheres; Daniel Bernouilli, at 10,000 atmospheres; and Dr. Hutton, at 2000. Count Rumford endeavours to prove in this paper that the force is equivalent to 50,000 atmospheres. But his mode of reasoning is not correct. Twelve grains of gunpowder were found to move a weight of 8800 lbs. out of its place. There is no difficulty in calculating the quantity of elastic fluid formed during the combustion of a given quantity of gunpowder, supposing the whole of it to be consumed. 100 grains of gunpowder contain usually about 67·3 grains of nitre. This quantity of nitre contains 35·8 grains of nitric acid, equivalent to 78 cubic inches of oxygen gas and 32 cubic inches of azotic gas, making a total of 110 cubic inches. Now as gunpowder is rather lighter than the same bulk of water, we shall not err much if we suppose 100 grs. of gunpowder to occupy the bulk of $\frac{1}{3}$ of a cubic inch; so that the gas in the powder is equivalent to 330 atmospheres. This gas is

chiefly converted into carbonic acid gas, which does not alter the bulk of the oxygen. We do not know the heat generated by the combustion; but it cannot be less than 1000°. Such a temperature would just triple the elasticity of the generated gas, and therefore render it equal to 990 atmospheres; so that, according to this supposition, the calculation of Robins is exact. But probably the heat may considerably exceed 1000°; and there is reason to conclude from Count Rumford's experiments that the sulphuret of potash which remains after the explosion of the gunpowder is at first in a gaseous state. All these causes must render the elasticity considerably greater than 1000 atmospheres. Probably Daniel Bernoulli's supposition is not very far from the truth.

The diminution of elasticity which took place when the elastic fluid generated from powder was prevented from escaping, and the formation of the hard stony substance, which astonished Count Rumford so much, is easily explained. The dissipation of the heat would speedily reduce the elasticity to one-third. Potash is capable of absorbing nine-tenths of its weight of carbonic acid. Therefore a considerable portion of the carbonic acid (the principal gas generated) would be absorbed, which would diminish the elasticity still farther. The stony body was a mixture of bicarbonate of potash and sulphuret of potash, the last of which would speedily absorb moisture from the atmosphere, and generate sulphureted hydrogen. —Count Rumford's notion that the elasticity of the elastic fluid from gunpowder is partly owing to steam is certainly erroneous. Nitre contains no water of crystallization. The charcoal that answers best for gunpowder is that which absorbs the least moisture. The drier the gunpowder is made, the stronger it is found to be. Hence it follows that moisture, instead of increasing, very much diminishes, the strength of gunpowder.

9. An Inquiry concerning the Source of the Heat which is excited by Friction. Phil. Trans. 1798. P. 80.—This is also a very curious paper. He found that by the friction of a steel borer against gun metal, pressing against it with a force of 10,000 lbs., while the gun metal turned round 32 times in a minute, 18¾ lbs. avoirdupois of water were made to boil in two hours and a half. The heat produced was as great as would have been given out by nine large wax candles burning with a clear flame all the time. He showed that this heat was not owing to any change in the specific heat of the metal, nor was it derived from the air. As no source could be pointed out, he draws as a conclusion, that heat is not a substance, but mere motion. But such a conclusion is going rather further than the experiments warrant. There is nothing absurd in supposing that friction has the property of drawing heat continually from the surrounding bodies, just as it does electricity, though it is not in our power to explain how it produces this effect.

10. On the Chemical Properties that have been attributed to Light. Phil. Trans. 1798. P. 449.—When certain substances are

exposed to the rays of the sun they undergo remarkable changes. Thus the oxides of gold and silver, if in contact with combustible bodies, are reduced; chlorine and hydrogen gas explode, and form muriatic acid; water holding chlorine in solution emits oxygen gas, &c. Count Rumford suspected that these effects were occasioned solely by the heat evolved by the absorption of the light. The experiments related in this paper were instituted in order to determine the point. Though they cannot be considered as quite satisfactory, yet it seems established by the subsequent experiments of chemists, particularly of Gay-Lussac and Thenard, that the opinion entertained by Count Rumford on this subject is correct.

11. An Inquiry concerning the Weight ascribed to Heat. Phil. Trans. 1799. P. 179.—From an experiment of Dr. Fordyce, it was concluded that bodies become heavier the more they are cooled, and of consequence that heat diminishes their weight. But Count Rumford found, on repeating the experiment, that the supposed increase of weight was a deception, arising from vapour condensing on the surface of the glass vessel in which the experiment was made. Lavoisier had previously ascertained the same thing.

It does not seem necessary to give a particular account of the remainder of Count Rumford's writings. His two volumes of essays are of a very miscellaneous nature, and the most important of the essays are republications of those papers which have been already noticed. The seventh essay, in which the Count endeavoured to prove that fluids are nonconductors of heat, has been sufficiently refuted by the more decisive experiments of subsequent chemists. Indeed the Count himself, though abundantly obstinate, appears at last to have given up his opinion. The essays on the treatment of the poor, on cooking, on chimnies, and on the management of fuel, are not very susceptible of abridgment. His paper published in the Philosophical Transactions for 1804, entitled, An Inquiry concerning the Nature of Heat and the Modes of its Communication, gives us a number of curious facts respecting the effect of surface on the heating and cooling of bodies. But the publication of Mr. Leslie's book on heat, in which this subject is treated of at much greater length, and much more completely, have deprived this essay of most of its interest. It is not necessary to notice the papers published by the Count in Nicholson's Journal for 1805. An outline of his last paper, On the Quantity of Heat evolved during the Combustion of different Bodies, was given in the first paper in the third volume of the *Annals of Philosophy*, to which I beg leave to refer the reader.

ARTICLE II.

On Dew. By Dr. Wells.

(To Dr. Thomson.)

SIR,

My Essay on Dew has lately been honoured in the Quarterly Review with a Criticism by Dr. T. Young, the depth and variety of whose knowledge entitle him, perhaps, to be considered as the most learned man in this country. I mention his name thus openly, because I am confident, that he has too much of the spirit of an English gentleman ever to desire to conceal his being the author of any publication, in which he discusses the merits of a literary work of another private person. With respect to the criticism of my work, indeed, he clearly shows, that he is the writer of it, by the manner, in which he speaks of his own works at its close.

I am necessarily much pleased with the general commendation, which has been bestowed upon my Essay by one of his high rank in literature. As several of his observations, however, respecting it do not appear to me entirely just, I beg leave to make a reply to these through the medium of your Journal.

I. Dr. Young has called my theory a simple and *obvious* consequence of principles deduced from the discoveries, concerning heat, by Mr. Leslie, and other observers. On this point I shall offer a few remarks.

1. The Inquiry of Count Rumford, and the Essay of Mr. Leslie, were both published in 1804, and in these works are to be found all the new facts relating to heat, which I have taken from others in forming my theory of dew. Whether Count R. ever afterwards treated of atmospherical appearances is unknown to me; but Mr. L. published, nine years after his Essay, a work on heat and moisture, in which, agreeably to the opinion of Aristotle, the production of dew is attributed to the condensation, by the cold of the night, of watery vapour diffused through a considerable portion of the atmosphere. Now, when the great ingenuity of Mr. L. is considered, if the theory of dew, which I have proposed, be an *obvious* consequence of his own discoveries, it would assuredly have occurred to him, in that long space of time, since he has shown, that the subject of dew had in the meanwhile occupied his attention.

2. Your own various publications demonstrate, both that you are well acquainted with the modern discoveries respecting heat, and that you have attended closely to atmospherical appearances; yet I remember, that you asked me, at an accidental meeting, shortly before the publication of my Essay, what my opinion was on the formation of dew, giving as a reason, that you were yourself ignorant of its cause.

3. Even Dr. Young, though his Lectures on Natural Philosophy prove him to have been well skilled in the new doctrine of heat, has advanced in that work an opinion upon the cause of dew, in the most ordinary form of its occurrence, which has not the least connection with any modern discovery, as I shall more particularly mention hereafter.

I might proceed in this way considerably further; but what has been said is, I think, sufficient to establish, that, when regard is paid to the imperfection of the human understanding, my theory of dew is not an obvious consequence of the recent doctrine of heat. My explanation, indeed, of the *immediate* cause of dew is altogether independent of that doctrine, being grounded on the simple fact, that bodies always become colder than the neighbouring air before they are dewed, and was consequently open to the discovery of every person since the invention of thermometers. It is true, that the next step in my theory could not have been taken, without the assistance of the late discoveries of others, and this has been amply acknowledged in my Essay.

II. "Dr. Wells," says Dr. Y., "appears, in his historical account of the doctrines relating to the nature and causes of dew, to have undertaken to afford us complete information respecting the sentiments, not only of Aristotle and Theophrastus, but also of the "most distinguished" philosophers of modern times: some of the works, however, of the persons whom he mentions, and some of the latest, have most unaccountably escaped his attention." In this sentence there are two small mistakes, which, from my respect for the author, I must suppose unintentional, though they give point to his statement. The first relates to the engagement into which he regards me to have entered concerning the opinions of preceding writers on dew; for I never gave the slightest hint, that could lead him to the conclusion which he has made. I knew that my account of opinions on the cause of dew was incomplete; and well it might be, since the whole of it, with the accompanying refutations, does not occupy more than three pages of my Essay. But my health, at the time of its being drawn up, was in such a state, that I scarcely hoped that I should ever finish my work, and my notes were so written, that no person besides myself could make use of them. I composed therefore in haste, and had neither leisure nor strength to search public libraries for all the works, which I wished to consult. I certainly thought, however, that what I had collected contained every thing of much importance, which had been said upon my subject. The second mistake consists in his applying the words "most distinguished," which were used by me concerning the authors, who had given opinions on the formation of ice in India, to those who had treated of dew.

I pass now to more important matters. Dr. Y. in support of what he has said respecting some very late and important works, connected with my subject, having most unaccountably escaped my attention, gives a long extract from the "Recherches sur la Chaleur" of Mr. Prevost, published in 1792. This work was, indeed, unknown to me when I composed my Essay; but Dr. Y. I presume was ignorant, when he wrote his Criticism, that the very passages, which he has cited, are contained in another and much later work on heat by Mr. P. unaccompanied with any intimation, that they were copied from a preceding publication. Mr. P.'s later work, which was printed in 1809, was first seen by me in 1812, two years before my Essay came out. What appeared to me the most worthy of attention in the passages cited by Dr. Y. was spoken of in one of my notes, p. 79. I mentioned there, very distinctly, that Mr. P. had already accounted for the effect of clouds, in diminishing the cold of the air at night, by making this to depend upon their preventing the escape of its heat by radiation to the heavens, but only impliedly, that he had accounted, in this way also, for a similar effect produced by them upon the temperature of bodies on the surface of the earth, as I said only, that he did not seem to have known, that they have a much greater effect upon the temperature of such bodies, than upon that of the air. My full meaning was, that Mr. P. did not seem to know, that the degree of cold, which is prevented by clouds from appearing on the surface of the earth, is much greater, than that which they prevent from appearing in the air; or in other words, that he was ignorant, that, bodies on the surface of the earth become much colder than the air in a clear night, this being one of the principal facts, on which my theory of dew is built. That I had no desire to conceal any thing which Mr. P. had said upon this subject is shown, by my referring to the latest work, in which he has mentioned the effect of clouds upon the heat of the earth and atmosphere at night, and by my referring, likewise, in three different parts of my Essay, (p. 68, 74, and 118,) to Count Rumford as supposing, that the earth is cooled by radiation at night; since it cannot be thought, that, although unacquainted with both of those authors, I should withhold the knowledge of the possessions of one, and yet repeatedly speak of similar possessions of the other.

I have said in the preceding paragraph, that Mr. P. did not seem to know, that the earth ever becomes colder, at its surface, than the air by radiation. My reasons are, 1st, That he has not mentioned this fact: 2dly, That he has said what is equivalent to a denial of it in his late work, Du Calorique Rayonnant, p. 249: and 3dly, That it is apparently in opposition to an observation of his friend Mr. Pictet, from whom he seems to have derived all the *facts* which he has related in this disquisition on the effect of clouds; for that philosopher found, that, although, in clear and calm nights, the heat of the air *decreased* from the height of 75 feet above the ground to within four lines of it, yet a thermometer, lying *upon* the ground, and having its bulb slightly covered with earth, "precisement enterrèe," was higher than all those which were suspended in the air above it.—*Pictet sur le Feu, p.* 180.

Considering the purpose to which Dr. Y. has applied his quotation from Mr. P., I shall venture to examine this a little closely.

1. What has hitherto been called by me Mr. P.'s *explanation* of the effect of clouds, at night, upon the temperature of the surface of the earth and of the lower atmosphere, is in reality only a *conjecture*; for, 1st, It is denominated by himself an "Essai d'Explication:" 2dly, It is supported by no experiment: 3dly, It takes as established, that air can radiate heat, whereas he says in p. 24 of his last work, "On peut *supposer*, que les molecules de l'air rayonnent."

2. Mr. P. mentions, that clouds send back to the earth a *little* more heat than transparent air can do; which is equivalent to affirming, that clouds alter the temperature of bodies upon the surface of the earth at night only a little. Nothing can show more strongly Mr. P.'s want of practical knowledge upon this subject; since I have given in my Essay, p. 32, an instance of the temperature of grass having risen 15° in less than 45 minutes, on the sky becoming cloudy.

3. It is remarked by Mr. P. that the operation of clouds, in keeping the surface of the earth warm at night, is exactly similar, "exactement comparable," to that of cloathing on the human body; and Dr. Y. assents to this observation. Now it appears to me, that very dissimilar things are here confounded together. Cloaths keep us warm by being bad *conductors* of heat; but clouds warm the earth by *radiating* heat to it. Hence, the effect of clouds is *immediate*, but cloaths require to be applied some time, before they completely answer their purpose. Cloathing must touch the skin to produce its full effect, whereas clouds produce theirs at the greatest distance. The thicker our cloathing is, the warmer it renders our bodies; but, if a cloud be sufficiently dense to prevent the heat, that is radiated into it, from passing through its interstices, the earth is kept as warm by it, as far as the principle of radiation is concerned, as it would be by one many times deeper. This is shown by a piece of cambric preventing the occurrence of cold on the earth's surface, from radiation, as effectually as a thick blanket.

4. Annexed to the disquisition of Mr. P., are two meteorological facts communicated to him by Mr. Pictet. One is, that upon a night in January, 1777, a thermometer, suspended in the open air, rose nearly 3½° of Fahr. in the space of an hour, the weather having in the mean time become cloudy. This was observed by himself; but it does not appear, that he afterwards made any similar observations. The second is given on the authority of husbandmen, who are said constantly to find that, though other circumstances are favourable for the production of dew, none, or almost none, appears, if the sky be cloudy; and that hoar frosts, which are so frequently injurious in spring and autumn, do not occur in those seasons, during cloudy weather. These facts, if

considered in themselves, are certainly very important, but they are not new, though they appear to have been thought so by Dr. Young, both by their being brought in support of his position, that I had been anticipated in several of the opinions contained in my Essay, which I had regarded as original, and by the use, which he has made of the Italick character, in presenting Mr. Pictet's words to his readers. With respect to the first fact, I referred in p. 78 of my Essay to Mr. Wilson's being acquainted with it, and as his observations concerning it appeared in 1786 in the Edin. Phil. Trans. his claim to the discovery must, by the common rule in such cases, be held superior to that of Mr. P., whose communication upon it was not published till 1792. Besides, Mr. W. formed his conclusion from several observations, whereas Mr. P. has given only a single one. But the same fact, though in forms somewhat different, was known to two authors anterior to both Mr. W. and Mr. P. For Lord Bacon says, Nat. Hist. § 866, "Star-like nights, yea, and bright moonshine nights, are *colder* than cloudy nights;" and Professor A. Wilson, of Glasgow, has mentioned, Phil. Trans. 1771, that on a winter night, during which the atmosphere was several times misty and clear alternately, he observed a thermometer, suspended in the air, always to rise from a half to a whole degree, whenever the former state began, and to fall as much, as soon as the weather became serene. In regard to the facts related by Mr. P. on the authority of husbandmen, I shall only say, that Aristotle expressly mentions, that neither dew nor hoar frost occurs except the night be clear, and that the same observation has been made by many other authors prior to Mr. P.

5. Mr. Prevost says at the end of his disquisition, that all the facts mentioned by Mr. Pictet may be accounted for on the principles previously established by him; but, as I have proved, I think, that he did not know that the surface of the earth ever becomes colder than the atmosphere, by radiation, any explanation he could offer of those facts must necessarily be imperfect.

Having now, I think, shown, that nothing is contained in the extract given by Dr. Y. from Mr. Prevost that required being taken notice of in my Essay, besides what he has said on the effect of clouds on the temperature of the earth and air at night, I shall mention, in justice to this philosopher, that the Essai d'Explication, just discussed, has risen considerably in my estimation, since I knew, that it had been given to the world so long ago as in 1792; for as I had before supposed, that he had been assisted in forming it by the works of Count Rumford and Mr. Leslie on heat, which were published in 1804. His opinion, therefore, on the effect of clouds in keeping the earth, and lower atmosphere, warm at night, appears to me now not only to be entirely original, but to exhibit a degree of knowledge on the subject of radiant heat, which I did not conceive had been possessed by any person at the time of its publication.

III. I mentioned in a note in the 105th page of my Essay, that

I had once intended to offer an explanation of some very curious observations on dew by a French author, Mr. B. Prevost, but that; fearing to prove very tedious, I had afterwards given up the design. I intimated, however, that my explanation would have been, in great measure, derived from the doctrine of radiant heat. On this note Dr. Y. has remarked, that if I had been as solicitous to attend to the labours of my contemporaries, as I had been laudably anxious (the expressions are his) to recur to those of my predecessors, I might have said, not that the experiments of Mr P. *might* be easily explained, but that they actually *had* been explained in a similar manner by one of my own countrymen, that is, by himself.

If Dr. Y. ascribes to me here a designed inattention to what he has written upon Mr. P.'s experiments, he is very much in error; but, if he means only, that, from want of sufficient diligence, I had never read the explanation given of them in his Lectures, he is, I believe, perfectly correct. For I do not recollect my having seen that explanation before I met with it in his Criticism, and I have no note of it among my papers, though these contain a considerable number of references to his 51st lecture, and one to the 60th. The cause of my not having examined an intermediate one, which contains the explanation spoken of, I take to be this, that having gone to a public library, with the view of consulting his work, while in haste, for the reason formerly mentioned, to finish my Essay, and not finding in the Index a single reference under the word "Dew," to any of his lectures, I searched no further.

But, admitting that I had been acquainted with Dr. Y.'s explanation of Mr. P.'s experiments, I should not have been prevented by that circumstance from offering one myself, if, from other reasons, I had been inclined to do this; since he treats of only a *part* of those experiments, and leaves unanswered several important questions relating to this part; whereas I should have treated of the *whole* of them, and have attempted to answer *all* the questions to which they give rise. I proceed now to justify the opinion which I have thus ventured to give respecting the inadequacy of Dr. Y.'s explanation. In the prosecution of this undertaking, I shall, in consequence of the indefinite manner in which the explanation is expressed, endeavour to show from collateral circumstances what it cannot be, rather than prove in a direct way what it is not.

The experiments, considered by Dr. Y., comprise some of the most important facts relating to the formation of dew; he, therefore, that can account for them fully, must, in my opinion, possess the true theory of that appearance. But that this was not the case with Dr. Y., when he published his lectures, is proved by the following passage in the very lecture, which contains the explanation in question. "The dew, which is deposited upon vegetables, is partly derived, in the evening, from the vapours ascending from the heated earth, [whence is the other part?]—and towards the morning from the moisture *descending from the air above, as it* *begins to cool.* Sometimes, however, in warmer weather, the dew begins to *descend* in the evening." For, what connexion with the doctrine of radiation has this theory of the dew, which is deposited by the atmosphere, and in what does it differ from that which was invented by Aristotle? In like manner it may be said, that if Dr. Y. had entertained a just notion of the cause of dew, he would not have conjectured, that the remarkable cold observed by Mr. Wilson on snow, in conjunction with hoar frost, a substance admitted by Dr. Y. to be frozen dew, might arise from the contact of air occasioning the snow to melt more readily; ii. 395; nor have supposed, that, when the surface of a metal remains dry, in the neighbourhood of a piece of glass which is covered with moisture, this arises from the different attractions for moisture in those bodies while of the same temperature; i. 708; since both of these facts must now, I think, if he considers them fully, appear to him to owe their explanation to the doctrine of radiation of heat. Having offered this argument in the first place, as being of easy comprehension, I pass to others of a different kind, which I am afraid I shall not be able, for various reasons, to render very perspicuous. To facilitate reference, I shall place at the bottom of this page all that he has transcribed in his Criticism from his Lectures concerning Mr. P.'s experiments.*

The account given by Dr. Y. of such of Mr. P.'s experiments as he means to consider, is, to my apprehension, involved in much obscurity, part of which would have been prevented by his mentioning, that the experiments were made upon a glazed window-frame, pertaining to a room the air in which was warmer than that without. But, notwithstanding that these circumstances were known to me, I mistook one of his facts on the first reading of his statement, and was not undeceived, until I consulted Mr. P. In Mr. P.'s apparatus a thin rectangular plate of metal is attached to the inner or outer side, or the warm or cold side, (to use Dr. Y.'s expression,) as occasion may require, of a pane of glass of the window, in such a manner, as to be surrounded by a margin of bare glass. When, therefore, Dr. Y. spoke of moisture being deposited on the glass *near* the metal, I naturally concluded, as I remembered the construction just described, that the portion of glass meant was, the naked margin on the same side with the metal. But on looking into Mr. P. I found, that Dr. Y.'s meaning must be, that the moisture was deposited on the glassy surface immediately *opposite* to the metal. If this interpretation be admitted, his statement, as far as it reaches, seems to me quite correct, except where it is said, that, when the metal is on the warmer, or inner, side, the humidity is deposited more copiously *either* on the metal, *or* on the glassy surface opposite to it; since, according to Mr. P., it may appear on both at the same time.

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* "It has been observed, that a piece of metal, placed on glass, usually protects also the opposite side of the glass from the deposition of dew; and Mr. Benedict Prevost has shown, that, in general, whenever the metal is placed on the warmer side of the glass, the humidity is deposited more copiously, either on itself, or on the glass near it, [as in the case of the shutter]; that when it is on the colder side, it neither receives the humidity, nor permits its deposition on the glass; but that the addition of a second piece of glass over the metal destroys the effect, and a second piece of metal restores it. It appears, that, from its properties with respect to *radiant heat*, the *metallic surface* produces these effects by preventing *ready communication* either of heat, or *of cold* to the glass."

My remarks on Dr. Y.'s explanation will chiefly refer to what takes place on the colder or outer side of the window. This learned author has said, that, when the metal is placed upon the warmer or inner side of the glass, the humidity is deposited *more* copiously on its outer surface, but has not mentioned where the *less* humidity occurs. He has compared, therefore, a thing that is known with something that is unknown, as far as his account of facts is concerned. This deficiency is to be supplied from Mr. P., who informs us, that the *less* humidity occurs on glass similarly exposed with the former, but having no metal attached to its inner surface. The appearance, consequently, on the bare glass, from being influenced by fewer circumstances, and being that with which the other is compared, is to be regarded as the standard fact. But surely we must be able to explain a standard fact, before we can fully account for any deflection from it. No sufficient reason, therefore, could, in my opinion, have possibly been offered by Dr. Y. for the greater quantity of moisture on the outer surface of glass, to the inside of which a metal was affixed, while he was ignorant, why moisture appeared in *any* quantity on bare glass similarly placed with the former, which I must conclude he was, from the reason given by him for the formation of dew upon vegetables, the relations of these substances and of glass to that fluid being the same, or nearly the same.

But it is manifest to me, for another reason, that Dr. Y. could not have given an adequate explanation of the facts related by him from Mr. P., which is, that he was unacquainted with certain conditions necessary for the appearance of those, which occur on the outside of the window, for they are not described by Mr. P., and Dr. Y. seems never to have made any experiments himself upon this subject. The conditions are, 1. The exposure of the window to a considerable extent of the heavens: 2. Stillness of the air: 3. Clearness of the sky.

Again:—The heat of the air in the room is a given condition; but whence is the radiant cold, to use an expression rendered necessary by Dr. Y.'s mode of accounting for the appearances, which enables the outer surface of the glass to condense the watery vapour of the atmosphere? It cannot be furnished by the neighbouring air, since this body must be unable to give more than what is possessed by itself, and unless the glass becomes colder than the air, the latter will not deposit moisture on the former. If it be now said, that the outside of the window-glass becomes colder than the air by receiving cold, which is radiated to it by the heavens, to employ still an expression authorised by Dr. Y., it will be replied, that this source of cold cannot be resorted to for the removal of the present difficulty, since, I think, I have shown, that Dr. Y. was ignorant of the necessity of a clear sky for the success of any of Mr. P.'s experiments, and unless the sky be clear, the outer surface of the window-glass will not be rendered colder than the atmosphere by radiation.

To conclude the consideration of Dr. Y.'s claim to having justly accounted for the facts related by Mr. P., I shall remark, that although his explanation could be made to apply to the whole of them, and all their attending circumstances, still, as it was never verified by experiments, that would admit the existence of no cause of the appearances observed, but that which is assigned by him, it can strictly be regarded in no other light than in that of a conjecture.* The conjecture, however, was, I believe, original; it was most happy too, since, if admitted to be just, it completely accounted for several important circumstances in Mr. P.'s experiments. If then its learned and ingenious author had established its truth by facts clearly seen by himself, and had afterwards pursued the subject of dew through its various ramifications, by means of the clue which would have thus been obtained, he must soon have acquired a knowledge of the theory, which has lately been submitted by myself to the consideration of the learned, and which he, as a member of that body, has pronounced to be just. But, I must, on the other hand be permitted to say, that, if Dr. Y., forgetting that Newton became a glass-grinder in the service of science, will neglect to employ, for the increase of natural knowledge, the slow and laborious method of observation and experiment, and will frequently exhibit his speculations in a manner, unsuited to the capacities of ordinary men, he ought not to think it strange, that opinions, advanced by him on difficult points of philosophy, are not, agreeably to his own remark at the end of the Criticism, received as truths beyond doubt, and are often not understood. I am, Sir,

Your most obedient humble servant,

London, Feb. 15, 1815. WILLIAM CHARLES WELLS.

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* In p. 475 of the second volume of Dr. Y.'s work on Natural Philosophy is the following passage:—"*Most* of the facts [related by Mr. P] may *perhaps* be explained by Mr. Leslie's discoveries." Hence I naturally concluded, that the author himself at the time of his publishing that work, regarded his explanation, not only as a conjecture, but as a conjecture inapplicable to the whole of the facts observed by Mr. P. I have very lately learned, however, that Dr. Y.'s *first* volume was printed *after* the *second*. What is said, therefore, by him, respecting the cause of those facts, in the passage quoted in his Criticism from the *first* volume, must be held to supersede what is cited by me in the beginning of this note from the *second*.

Article III.

Experiments to determine the Definite Proportions in which the Elements of Organic Nature are combined. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

III. *Analysis of the Ternary Oxides.*

(*Concluded from* p. 184.)

Substances not Acid.—I mentioned before that the ternary oxides composed of carbon, hydrogen, and oxygen, have a strong tendency to combine with saline bases; and that in these combinations they act the part of acids. They possess the acid characters, however, in very different degrees. Many of them possess the property of combining at the same time with the strong acids, as is the case with tannin, and almost all the extractive and colouring matters. These oxides then are placed near the middle of the electro-chemical chain, and their affinities diminish in proportion as they approach the *point of indifference* of that chain. The only ternary oxide which contains nitricum, is a strong alkali; and the quaternary oxides, which contain nitricum, have likewise properties opposite to those of vegetable substances. They lie on the other side of the point of indifference of the electro-chemical chain; their tendency to unite with acids being more distinct than to combine with bases.

The ternary oxides already analyzed are all of a decided electro-negative character, so that chemistry gives them the name of acids; if we except tannin, the acid properties of which are however very distinct.

It is known that the resins, fat oils, gums, extracts, &c., form insoluble compounds with various metallic oxides. These combinations are generally stated in such a manner in our Manuals of Chemistry, that a reader, whose attention is not specially directed towards these objects, considers these combinations as the only ones of which these organic bodies are capable: and, in fact, they are the only ones which are recognized by some striking property. These insoluble combinations of ternary oxides with binary oxides belonging to the class of salifiable bases, makes it probable that there exists a general affinity between these ternary oxides and bases. It is easy to convince ourselves of the truth of this suspicion by experiment.

Gum is precipitated by subacetate of lead, and sugar has the property of rendering lime more soluble in water. These facts were ascertained without the consequence being drawn from them, that these combinations owe their existence to a general affinity between saline bases and vegetable bodies, in consequence of which gum ought to have an affinity with lime and sugar for oxide of lead.

If we pour sugar into a solution of subacetate of lead, no precipitate takes place. But we should deceive ourselves, were we to conclude from this, that sugar does not combine with oxide of lead; or that it does not form an insoluble compound with that oxide. A solution of sugar not only dissolves oxide of lead; but by a long digestion it combines with an excess of that oxide, and forms an insoluble, light, white, and bulky compound. This compound dissolves in acetate of lead, subacetate of lead is formed, and the sugar is disengaged. I shall have occasion to speak more of this combination below. If we pour some drops of caustic ammonia into a solution of sugar of milk, we do not observe any change. The ammonia acts as a re-agent, just as if the sugar of milk were not present. We should deceive ourselves were we to conclude from this that sugar of milk and ammonia have no affinity for each other. We have only to macerate at the temperature of 122° a solution of sugar of milk with oxide of lead, and then to drop a little ammonia into the filtered liquid. This liquid, which is a combination of sugar of milk and oxide of lead, is decomposed, and an insoluble compound of sugar of milk with an excess of oxide of lead is precipitated. This precipitation is owing to the affinity of ammonia for sugar of milk, which it divides with the oxide of lead.

It is, in general, very difficult to obtain neutral combinations with these substances; at least unless this can be done when they are in solution in water, which has the same action on them as on the metallic oxides placed round the point of indifference of the electro-chemical chain; as the oxide of bismuth, antimony, tellurium, &c. We know that it is impossible to obtain a neutral muriate of these oxides, by treating them with liquid muriatic acid; we obtain only an insoluble submuriate and a soluble muriate with an enormous excess of acid. In the same manner when we digest a solution of common sugar, or sugar of milk, with oxide of lead, we obtain only an insoluble compound with an excess of oxide of lead, and a soluble combination with an enormous excess of sugar. If you pour ammonia into a solution of sugar, nothing indicates that the substances combine, and the ammonia evaporates from that solution as easily as from pure water. But if you expose sugar in powder to the action of ammoniacal gas, the sugar absorbs the gas, and forms with it a compound. Here the same thing takes place as when you expose oxide of antimony to the action of muriatic acid gas. These observations, I conceive, not only prove that the ternary vegetable oxides have a general tendency to combine with salifiable bases; but they point out also the reasons why this general tendency has hitherto remained unnoticed.

To be able to speak of these combinations it will be necessary to give them names, and I thought it would be agreeable to the principles of the chemical nomenclature to name, for example, a combination of sugar with oxide of lead, according to the different degrees of saturation, *saccharate*, *sub-saccharate*, *super saccharate* *of lead.* In the same manner I shall use the terms *gummates, amylates, saccolactates,* to denote the combinations of gum, starch, sugar of milk with saline bases.

There is still a circumstance relative to the combination of the ternary oxides with the saline bases, which deserves to be mentioned here. An excess of these bases, especially the stronger ones, decomposes a great part of the ternary oxides in different ways, usually producing a great quantity of carbonic acid. The ternary oxides least exposed to this decomposition are these: 1. whose atoms of oxygen are equal in number to those of some one of the other elements, or surpass them in number: and 2. when the ratio of the hydrogen to the oxygen is less than in water. Such is the case with most of the acids analyzed. If the atoms of carbon and hydrogen surpass in number those of oxygen to a certain amount, the ternary oxide has a great tendency to undergo decomposition from the action of the salifiable bases. Such, for example, is the case with gallic acid, tannin; and, to a certain extent, with acetic acid. When, on the other hand, the atoms of each of the combustible elements surpass four or five times those of oxygen, the ternary oxide is more permanent: so that those oxides are best preserved which have either a great excess of combustible atoms or of oxygen. Of consequence, benzoic acid and the fat oils are but insensibly altered by the action of alkaline bodies.

This is the reason why, when the ternary oxides are gradually decomposed, either by the influence of air and water, or of acids, their atoms arrange themselves in such a manner as to form new products, in which, on one side, the oxygen is in excess, and, on the other, the atoms of the combustible elements greatly surpass those of the oxygen. Thus sugar is converted into carbonic acid and alcohol by fermentation; and gallic acid, by the influence of an alkali, yields on one side water and carbonic acid, and, on the other, an extractive matter abounding in carbon, to which it owes its dark colour. Perhaps it would be useful to class the ternary oxides into: 1. Oxides not easily decomposed in consequence of their excess of oxygen; such as oxalic, citric, tartaric acids, &c. 2. Oxides easily decomposed in consequence of the equilibrium of their elements, which is easily destroyed by a small force; such as tannin, gallic acid, sugar of milk, common sugar, &c. 3. Oxides not easily decomposed in consequence of the excess of their combustible atoms; such as benzoic acid, resins, fat oils, &c.

Let us now turn our attention more particularly to the ternary oxides destitute of acid properties.

10. *Common Sugar.*

I digested a solution of sugar with oxide of lead. At first the oxide dissolved, but after the digestion had been continued for some time, it was converted into a light white powder, by which the whole liquid was rendered opake. I separated the white powder on a filter, washed it with boiling water, and dried it in a vacuum. This substance is a new combination of sugar and oxide of lead. It is quite insoluble in water, light, white, and destitute of taste. The acids, even carbonic acid gas, separate the sugar from it. When heated to a certain point it takes fire, and continues to burn of its own accord, leaving as a residuum oxide of lead mixed with metallic lead. It appears to contain no combined water. I have not been able to procure this substance quite free from carbonate of lead; but it is easy to determine how much of it is present by dissolving it in acetate of lead, which does not act upon the carbonate. By this means I found that in the subsaccharate of lead employed in my experiments, there was 1½ per cent. of carbonate of lead; for 10 parts dissolved in acetate of lead left 0·15 of carbonate of lead undissolved.

Two parts of the subsaccharate of lead, when burnt, left 1·1728 of oxide of lead; but we must subtract 0·03 for carbonate of lead, that is, 0·025 of oxide of lead, and 0·005 of carbonic acid. There remains 1·1478 for the oxide of lead, and 0·8222 for the sugar; so that the subsaccharate is composed of

Sugar	41·74	100
Oxide of lead	58·26	139·6
	100·00	

I repeated this analysis several times, and the results varied between 138 and 140 of oxide combined with 100 of sugar. The reason of this variation seems to be the difficulty of discovering when the oxide of lead is entirely penetrated with sugar. When any of it remains uncombined, it is obvious that the analysis will give an excess of base. The oxygen of 139·6 of oxide of lead is 9·98.

If we digest the above-mentioned subsaccharate in a solution of sugar, a part of it dissolves, and forms a clear liquid, with a slightly yellow colour, which contains lead. But the quantity of this metal is very small when compared with that of the sugar, of which the solution appears to contain an excess in the form of super-saccharate. When evaporated it leaves behind it a syrupy mass, which does not crystallize, and which attracts humidity from the atmosphere.

The crystalline form of sugar does not lead us to suspect that it contains water. I reduced it to a fine powder, and dried it in a vacuum. The loss of weight was only 0·1 per cent. I took ten parts of this sugar, and mixed them with 40 parts of yellow oxide of lead, reduced to a fine powder, and heated to redness afterwards. I digested this mixture in water, in the heat of a water-bath, till the oxide of lead had absorbed all the sugar. I then put it into a vacuum and dried it. The loss of weight was 0·53. I then heated it to 212° in a vacuum; but it sustained no farther loss of weight. This loss must have been water combined with the sugar; for, on dissolving the saccharate in nitric acid, not a single bubble of carbonic acid gas made its escape. This shows that the sugar had not undergone decomposition. From this experiment it appears that sugar in its ordinary state is a compound of

Sugar	94·7	100
Water	5·3	5·6
	100·0	

But 5·6 of water contain 4·941 of oxygen, which is exactly the half of that found in the oxide of lead with which the sugar is combined in the sub-saccharate.

I took five parts of sugar dried in a vacuum, and put them into a small glass exactly weighed, the mouth of which was covered with paper. I put this glass in a jar over mercury, and then filled the jar with ammoniacal gas. The gas was slowly absorbed; the sugar contracted in bulk, and its surface acquired so strong a crystalline lustre as to appear humid. The saturated combination was a dense, coherent, flexible mass, which might be cut with a knife. It exhaled the odour of ammonia. The absorption of ammoniacal gas continued for four days; but I left the sugar in the gas 24 hours after it was saturated. It had gained 0·26 of its weight. This combination, which I consider as a neutral saccharate of ammonia, is composed of

Sugar	90·000	100
Ammonia	4·930	5·49
Water of combination	5·070	5·60
	100·00	

But 5.49 ammonia contain 2·5 of oxygen, which is half the quantity that the water contains, and ¼th of the oxygen in the oxide of lead.

0·4 of sugar dried in a vacuum, gave by combustion from 0·237 to 0·24 of water; and from 0·607 to 0.61 of carbonic acid gas. Hence it follows, that sugar is composed of

Hydrogen	7·05
Carbon	41·48
Oxygen	51·47
	100·00

But we have seen that in these 100 parts of sugar there are 5·3 of water; containing 4·67725 oxygen. Now 4.67725 × 11 = 51·44975. Hence it follows, that the 94·7 parts of pure sugar contained ten times that quantity, or 46·7725, and consequently, that sugar contains ten times as much oxygen as the water, five times as much as the oxide of lead, and 20 times as much as the ammonia with which it was combined in the compounds mentioned above.

0·9675 of subsaccharate of lead, equivalent to 0·4 of sugar, produced by combustion from 0·230 to 0·231 of water; and from 0·650 to 0·652 of carbonic acid gas. This gives from 6·756 to 6·785 per cent. of hydrogen, and from 44·2 to 44·336 of carbon. According to this analysis sugar is composed of

Hydrogen	6·785
Carbon	44·200
Oxygen	49·015
	100·000

The difference between these two analyses is to be ascribed to the presence of water in the first analysis. It cannot be ascribed to the presence of carbonic acid in the subsaccharate employed; for this would occasion a diminution in the weight of the carbon found, as well as an augmentation of that of the oxygen, since sugar contains much more carbon and much less oxygen than carbonic acid.

But we have seen that sugar contains five times as much oxygen as the oxide of lead with which it is combined, and ten times as much as the water. Hence it ought to contain either five or ten volumes of oxygen. When we compare the weights of its elements with one another, we find that it is composed of 100 + 12 C + 21 H. This gives per cent.

Hydrogen	6·802
Carbon	44·115
Oxygen	49·083
	100·000

A result which comes very near the actual analysis. Some readers will probably think it likely that the 21 H ought to be either 20 or 24, in order to be double the number of atoms either of oxygen or carbon; but it is evident, that the difference in the quantities of water which ought to be produced by the combustion of sugar, according to either the one or the other of these suppositions, is so considerable, that it would make a very sensible difference in the result of the analysis. And we shall see afterwards, that such a proportion between the oxygen and hydrogen characterizes the substances which do not yield mucous acid when acted upon by nitric acid.

To obviate the objection that the saccharate analyzed might contain some other body besides sugar, I decomposed a certain quantity of it by mixing it with water, through which I passed a current of carbonic acid gas. I filtered the liquid, and concentrated it. It yielded pure white crystals, which possessed all the properties of common sugar.

I likewise burnt a part of it, and obtained from 0·4 of sugar, dried in a vacuum in a hot sand-bath; 0·2346 of water, and 0·628 of carbonic acid, which makes in 100 parts

Hydrogen	6·891
Carbon	42·704
Oxygen	50·405
	100·000

Now it is evident that this is 21 O + 24 C + 44 H; that is to say, that it contains 1 O + 2 H more than pure sugar, and as much less than the sugar which I have before analyzed; so that this last sugar contains only half the water of combination of the other. As this was the last of all the experiments described in this memoir; and as it was made just at the time when other circumstances obliged me to desist, I have not had an opportunity of repeating it. It would have been interesting to discover by what accident one half of the water of combination had been removed.

Thenard and Gay-Lussac found sugar composed of

Hydrogen	6·90
Carbon	42·47
Oxygen	50·63
	100·00

This coincides with my last analysis.

11. *Sugar of milk.*—The sugar of milk employed in the following experiments was purified by repeated crystallizations. It was then reduced to powder, and dried at 212° in a vacuum. Gay-Lussac and Thenard found sugar of milk composed of

Hydrogen	7·341
Carbon	38·825
Oxygen	53·834
	100·000

I burnt 0·4 of sugar of milk, which produced 0·244 water, and 0·5805 carbonic acid. From these data sugar of milk is composed of

Hydrogen	7·167
Carbon	39·474
Oxygen	53·359
	100·000

These numbers correspond almost exactly with the supposition that sugar of milk is composed of one volume of oxygen, one volume of carbon, and two volumes of hydrogen. According to that supposition it is composed per cent. of

Hydrogen	7·019
Carbon	39·819
Oxygen	53·162
	100·000

We can hardly doubt, therefore, that crystallized sugar of milk is composed of $O + C + 2H$. But it would be a mistake to suppose that pure sugar of milk has such a composition. It contains combined water as well as common sugar. If it be fused at a temperature not sufficiently high to decompose it, a great deal of water is disengaged, which is not formed by any alteration in the sugar, as it remains colourless and gives a colourless solution. To succeed in this experiment it must be made on a very small scale. Common sugar does not give out any water till it begins to become brown.

To determine the quantity of water in sugar of milk, I made an experiment similar to that made with common sugar. I took three parts of sugar of milk and mixed them with 20 parts of oxide of lead, and digested the mixture with a little water, till the whole sugar was absorbed by the oxide of lead, and the mass was reduced to the consistence of a thick paste. This mass, dried in a vacuum at the ordinary temperature of the atmosphere, lost 0·34 of its weight. When heated to 212° in a vacuum, it sustained an additional loss of 0·04, making in all 0·38, equivalent to 12.333 per cent. These 12⅓ contain 10·8 of oxygen, which is the fifth part of all the oxygen contained in the crystallized sugar of milk. The small deviation is too inconsiderable to be owing to any thing else than an error in the experiment. Hence it follows, that sugar of milk is combined with a quantity of water, which contains one fourth as much oxygen as itself. Experiment in that case ought to have given 12·04 of water, instead of 12·333.

To determine the capacity of saturation of sugar of milk I digested a mixture of it and oxide of lead in water, in a corked phial; the oxide had been obtained by burning oxalate of lead in an open vessel, because the oxide thus prepared is in the state of a very fine powder. On attempting to assist the solution of the oxide by heat, I found that the temperature must not be raised higher than 140°, because at a higher temperature the sugar of milk is decomposed by the action of the oxide of lead; the liquid becomes brown, and assumes an empyreumatic odour, though it has not been raised to the boiling temperature. Bouillon Lagrange, and Vogel, have already observed this property of sugar of milk to be decomposed by the more powerful bases.

A strong solution of sugar of milk, digested during eight hours with oxide of lead, gave a colourless milky solution, of a taste a little sweet, alkaline, and astringent. This solution contained a small quantity of a white, light, and mucous matter, which was easily separated from the oxide of lead undissolved, by agitating the vessel. I poured the solution, holding the mucous matter suspended upon a filter; and as the liquid passed very slowly, I placed it under a glass jar, into which I had introduced a quantity of moist hydrate of lime, in order to absorb the carbonic acid gas of the air, and prevent the liquid from being affected by it.

The mucous matter remaining on the filter had some resemblance to oxide of tin. When dried in a vacuum it was semi-transparent, and greyish coloured. When heated to 212° in a vacuum, it gave out water, and became yellow. This yellow matter when exposed to the direct rays of the sun, is decomposed in a few minutes. It becomes at first green, and then grey. Decomposed by combustion it gave 63·529 per cent. of oxide of lead. Hence it was composed of

Sugar of milk	36·471	100·00
Oxide of lead	63·529	174·15
	100·000	

These 174·15 of oxide of lead contain 12·45 of oxygen; but we have seen that sugar of milk contains 12¼ water, or 100 sugar of milk are combined with 14 water, in which there are 12·245 of oxygen. It is therefore clear that in the saccolactate of lead analyzed, the sugar of milk ought to contain four times as much oxygen as the oxide of lead.

A part of the filtered liquid mixed with caustic ammonia, deposited an abundant precipitate, possessing all the characters of the above-described saccolate, except the mucous aspect and the pellucidity after desiccation. It became equally yellow when the water of combination was driven off. Analyzed by combustion it gave 64·14 per cent. of oxide of lead. Treated by neutral acetate of lead it gave a little carbonate of lead, to the presence of which it probably owes its difference from the saccolate above described.

Another part of the liquid evaporated to dryness gave a gummy and transparent mass, which did not attract moisture from the atmosphere. When dried at 212° in a vacuum, it swells up, becomes frothy, and acquires a yellow colour. Analyzed by combustion it left 18·123 per cent. of oxide of lead. I consider this substance as a super-saccolate of lead. It is composed of

Sugar of milk	81·877	100
Oxide of lead	18·123	22·1

Thus it appears that in the super-saccolate the sugar of milk is combined with $\frac{1}{8}$ of the quantity of base as in the neutral saccolate. For $\frac{174 \cdot 15}{8} = 21 \cdot 77$. The error in the result is owing to the impossibility of drying the gummy combination without exposing it to a temperature at which it begins to undergo decomposition to which its yellow colour is owing.

Oxide of lead digested with sugar of milk appears at first not in the least altered. It is, however, converted into a subsaccolate of lead. When well washed, and dried in a vacuum in the temperature of 212°, it forms a yellow powder. When analyzed by combustion it leaves 87·2 per cent of oxide of lead. Hence it is composed of

Sugar of milk	12·8	100
Oxide of lead	87·2	681
	100·0	

But 174·15 × 4 = 696·6; hence it follows, that in the sub-saccolate the sugar of milk is combined with four times as much base as in the neutral saccolate.

As it is easier to obtain this last combination with excess of oxide in a state of purity than the others, I consider the result of its analysis as nearer the truth than that of the others. It is clear that the sugar of milk and oxide of lead in it ought to contain equal quantities of oxygen. 681 of oxide of lead contain 48·65; and we shall see immediately, from other data, that the sugar of milk ought to contain exactly the same quantity.

I exposed five parts of pulverized sugar of milk to the action of ammoniacal gas; their action on each other was very slow, especially at the commencement. The absorption continued for 15 days. The sugar of milk had then diminished a little in bulk, and had assumed a yellowish colour, owing to some slight alteration in its composition. It now weighed 5·62, and exhaled a strong smell of ammonia. I left it under a glass jar, not shut, and weighed it at the end of every half hour. It diminished in weight with a great deal of rapidity, so that after two hours it weighed only 5·33. After this I found that it scarcely lost any more weight; for in 18 hours the loss did not exceed 0·05. From this we see that sugar of milk forms at least two saccolates of ammonia, in which the proportions of ammonia are as 1 : 2. Now we have seen that 100 parts of crystallized sugar of milk contain 12⅓ per cent. of combined water, and that the same quantity of sugar of milk absorbs 12·4 of ammonia. The oxygen of this last is 6·7, and that of the first 10·9, or very nearly twice as much; so that we have here found the same ratio between the ammonia and the water of combination as in common sugar. And though the sugar of milk absorbed a little more ammonia than it ought to have done according to calculation, this seems to have been owing to a commencement of decomposition in it. In fact, when saccolate of ammonia was dissolved in water, and muriatic acid mixed with the solution, there was a disengagement of some carbonic acid, which sufficiently explains the excess of ammonia absorbed. I ought, besides, to make the general observation, that the tendency of sugar of milk to be decomposed by the bases, which appears to exceed that of tannin and gallic acid, prevents any of my experiments on its capacity of saturation from being decisive; but as they correspond sufficiently with the probable composition of sugar of milk, I consider them as probably very near the truth.

Supposing crystallized sugar of milk to be O + C + 2 H, and that it contains a quantity of water the oxygen in which is ¼ of the whole found in the substance, there ought to remain for pure sugar of milk 40 + 5 C + 10 H, or, according to the composition of saccolate of ammonia, 8 O + 10 C + 16 H, which constitute per cent.

Hydrogen	6·385
Carbon	45·267
Oxygen	48·348
	100·000

Sugar of milk then contains more carbon and less oxygen than common sugar. This explains why, when we burn the compound of these two bodies with oxide of lead, the first reduces much more lead than the other. We understand also why sugar of milk is more easily carbonized than common sugar.

12. *Gum Arabic.*—I mixed caustic ammonia with a filtered and boiling solution of gum arabic. I then mixed it with a solution of subnitrate of lead, likewise boiling hot. I took care not to add enough to precipitate the whole of the gum. I decanted off the liquid from the precipitate, which I washed repeatedly with boiling water. I then subjected it to pressure, and dried it in a vacuum, finally exposing it to the heat of 212°.

When analyzed by combustion, the gummate of lead left for residue 38·25 per cent. of oxide of lead. It was therefore composed of

Gum	61·75	100·000
Oxide of lead	38·25	62·105
	100·00	

The oxygen of these 62·105 of oxide of lead is 4·44. To obtain a subgummate of lead I digested a solution of gum with oxide of lead. A subgummate was formed, very heavy and coherent; but which could not be entirely separated from the gummy liquid. Of course I was unable to analyze it.

0·648 of gummate of lead, equivalent to 0·4 of gum, produced by combustion 0·2297 of water, and 0·617 of carbonic acid, equivalent to 6·75 per cent. of hydrogen, and 41·956 of carbon.

0·4 of gum arabic, in the state of a fine powder and dried in a vacuum at the temperature of 212°, produced by combustion 0·231 water and 0·6196 of carbonic acid, equivalent to 6·788 per cent. of hydrogen and 41·906 of carbon. Hence it apppears, that dried gum arabic contains no water; for it is probable that gummate of lead cannot retain water with greater force than the other combinations having oxide of lead for their base. Gum arabic is then composed as follows:

Hydrogen	6·788
Carbon	41·906
Oxygen	51·306
	100·000

But we see that gum arabic ought to contain 12 times as much water as the base by which it is saturated: for 4·44 × 12 = 53·28. This small difference ought to be ascribed to a small error in the analysis, or in the preparation of gummate of lead; which error is in the present case increased 12 fold in the calculation. But if gum arabic contain 12 atoms of oxygen it must contain 13 of carbon and 24 of hydrogen, which make in 100 parts

Hydrogen	6·792
Carbon	41·752
Oxygen	51·456
	100·000

This agrees very well with the result of direct experiments. Gum arabic then is 12 O + 13 C + 24 H.

The analysis of Gay-Lussac and Thenard gave 6·93 hydrogen, 42·23 carbon, and 50·84 oxygen.

I ought to observe, that in my experiments, gum, whether pure or combined with oxide of lead, produced traces of nitric acid; so that what in the analysis I considered as pure hydrogen, contains in fact a trace of the radicle of nitric acid; which, as is known, has the same capacity for oxygen that hydrogen has.

I am, however, far from pretending that gum arabic contains nitricum among its elements. It is more probable, that the formation of the nitric acid is owing to the presence of a small quantity of vegetable albumen in the gum. Probably the excess of $\frac{1}{2}$ of a per cent. of carbon obtained in the analysis, is owing to the same cause.

Gum arabic, when incinerated, leaves a small quantity of ashes, the weight of which is very variable; and, of course, it cannot be considered as an essential constituent of the gum. So much the less, as both Vauquelin and Cruikshanks found that gum contained vegetable salts with a base of lime. To see whether gum contained any secondary element, I endeavoured to separate it from gummate of lead. I found that this could not be accomplished either by means of sulphuric acid, or sulphureted hydrogen; for the disengaged gum causes the insoluble precipitates to pass through the filter, and it prevents them from being ever deposited. I therefore dissolved gummate of lead in acetic acid, and into the solution I poured alcohol, which precipitated the gum. I washed the gum very well in alcohol. This gum, being dried and burnt in a glass capsule, left oxide of lead, retained probably in the state of a super-gummate of lead. The ashes were easily soluble in acetic acid. The liquid being evaporated to dryness, and burnt, left sulphate of lime, with traces of sulphate of potash, weighing together $\frac{1}{3}$ of a per cent. of the gum. But sulphuric acid constituting more than one half of these ashes, it appears that the small quantity of the radicles of the bases cannot be considered as of any value in the composition of gum arabic.

Thenard and Gay-Lussac obtained 2·4 per cent. of the gum in ashes, of which they kept an account in the exposition of the result of their analysis of gum arabic. These ashes consisted chiefly of carbonate of lime, the carbonic acid of which constituted nearly one per cent of the gum. No attention was paid to this; so that what they considered as 100 gum, was in reality 101. Such an error is too considerable to be neglected.

Gum in its ordinary state contains water; but this water is a hygroscopic substance, whose quantity varies according to the state of the hygrometer. I have found that gum in powder, dried in a vacuum at the temperature of 212, lost about 17 per cent. of water. From this we may conclude, that gum ought to contain a quantity of water, the oxygen in which is $\frac{1}{4}$ that in the gum. I have not found that gum when dried slowly forms other combinations with water.

18. *Potatoe Starch.*—I took some potatoe starch, mixed it with a little water, and poured it by small portions at a time into boiling water, till I had obtained a solution of the requisite degree of consistency. I passed it through a linen cloth, and then mixed it with a boiling solution of subnitrate of lead, which I added in excess, and with which I digested the precipitate for a long time, that no part of the starch might remain uncombined. I then washed the precipitate by boiling it repeatedly in water, till that liquid ceased to take up any more subnitrate of lead. I then subjected it to pressure, and dried it in a vacuum in the temperature of 212°. This amylate, when analyzed by combustion, left 28 per cent. of oxide of lead. Hence it is composed of

Starch	72	100
Oxide of lead	28	38·89
	100	

These 38·89 parts of oxide of lead contain 2·78 of oxygen.

I endeavoured, likewise, to form an amylate containing more base. I found that such a compound exists, though I was not possessed of any method of separating it in a state of purity. We form it by mixing ammonia with a boiling solution of starch, and precipitating the mixture by a boiling solution of subnitrate of lead. But we cannot (unless by pure accident) find the point at which no neutral amylate is formed, or no subnitrate of the last degree. A subamylate, which I endeavoured to obtain as near the point of supersaturation as possible, without being mixed with insoluble subnitrate, gave by combustion half its weight of oxide of lead; so that it seems to contain the starch combined with three times as much base as in the neutral amylate.

0·4 of starch, dried in a vacuum, gave by combustion 0·2405 water, and 0·6395 carbonic acid, equivalent to 7·064 per cent. of hydrogen, and 43·481 of carbon.

0·555 of amylate of lead, equivalent to 0·4 of starch, produced by combustion 0·239 water, and 0·643 carbonic acid, which gives us per cent. 7·023 hydrogen, and 43·724 carbon. It appears then that starch dried in a vacuum does not retain any water. Potatoe starch then is composed of

Hydrogen	7·064
Carbon	43·481
Oxygen	49·455
	100·000

But we find that in amylate of lead the starch contains 18 times as much oxygen as the base, and in the subamylate six times as much. For 2·78 × 18 = 50·04, which differs very little from 49·455. Starch then contains either six or 18 atoms of oxygen. It is therefore composed of 6 O + 7 C + 13 H, or of 18 O + 21 C + 39 H. According to this statement, its composition should be

Hydrogen	7·090
Carbon	43·327
Oxygen	49·583

This agrees well with the analytical result.

Thenard and Gay-Lussac found wheat starch composed of 6·77 hydrogen, 43·55 carbon, and 49·68 oxygen. 20 parts of starch, when incinerated in a platinum crucible, furnish 0·046 of ashes, consisting entirely of the earthy phosphates. Of course they appear to have been only mechanically mixed with the starch; but if they be chemically combined, it is difficult to determine the relation of their elementary atoms to those of the primary elements.

Before finishing this first set of experiments on the chemical proportions in organic nature, I shall make some observations relative to the substances which have come under our review.

As to the number of elementary atoms of which an organic atom is composed, it does not seem necessary that the most simple ratio between the elements should express that number. Thus it does not seem necessary that an atom of starch ought to contain 6 O + 7 C + 13 H, or 26 elementary atoms; it is just as possible that it contains three times that number, and that it is upon such a composition that the proportion between the starch and oxide of lead in the neutral amylate depends. In the same way, we may say, that gallic acid is composed of 3 O + 6 C + 6 H, and not of O + 2 C + 2 H. The great number of elementary atoms combined in a compound atom does not constitute any objection to this conclusion; for if the atom of starch contains in fact 78 elementary atoms, we may, on the other side, point out in organic nature compound atoms of the second and third order, which contain a still greater number of elements. Thus the atom of crystallized alum contains one atom of potassium, two of aluminium, eight of sulphur, 80 of oxygen, and 96 of hydrogen, constituting altogether

187 elementary atoms: or, even supposing potash to contain only one atom of oxygen, the atom of crystallized alum will still contain 94 elementary atoms.

The organic atoms must be much larger than those of the first order in organic nature: hence the reason why their capacity of saturation is in general so small.

It is well known that sugar of milk and gum arabic form *mucous acid* when treated with nitric acid, while sugar and starch do not furnish any of this acid. In the two first the hydrogen has to the oxygen the same ratio as in water. The two last contain hydrogen in excess.

Sugar of milk is composed of........ 8 O + 10 C + 16 H
Mucous acid contains............... 8 O + 6 C + 10 H

If from the first you take 4 C + 6 H, you obtain the second. In the same manner, by taking from two atoms of gum arabic 4 C + 9 H, you will leave three atoms of mucous acid. As to sugar and starch, if you represent the first as containing 20 atoms of oxygen, and the last as containing 18 atoms, we shall find it impossible to form mucous acid without taking away some atoms of oxygen.

We may then form the idea that the organic atoms have a certain mechanical structure, which enables us to deprive some of them of certain elementary atoms without altering the whole very much. In this way we remove certain atoms of sugar of milk, and there remains mucous acid. If we continue to remove other atoms, this acid is likewise destroyed, and the whole reduced to the common products of the decomposition of the ternary oxides, namely, oxalic and malic acids. The construction of the atom of common sugar is such that, as soon as you take away the first elementary atom, it separates (so to speak) into oxalic and malic acids. It is only by such a structure that we can explain the different products from the action of nitric acid on the ternary oxides, composed of the same elements, and in proportions (stated in per cents.) but little different from each other.

I am persuaded that an attempt to study the probabilities of the construction of organic atoms, nearly in the same way as Dr. Wollaston endeavoured to represent the composition of boracite, by means of differently coloured balls—I am persuaded, I say, that such a study would be of the greatest importance, and might be even capable of correcting analysis; as analysis might probably indicate a number of elementary atoms, incapable of forming any regular figure whatever.

If we were acquainted with the composition of alcohol, and of several varieties of sugar, the theory of fermentation would follow as a consequence from this knowledge. It is natural to suppose that fermentation is a very simple operation, and that it connects merely in the formation of carbonic acid gas, from the carbon of the sugar uniting partly with the oxygen of the sugar, and partly with that of the water, the hydrogen of which combines with the rest of the sugar, and forms alcohol.

In my memoir On the Cause of Chemical Proportions, I mentioned organic substances as of a composition difficult to be explained by the views of the corpuscular theory. We see that in proportion as we study organic nature these difficulties disappear; and in the present state of our knowledge the corpuscular theory is the only one which puts it in our power to explain the composition of organic atoms in a satisfactory manner. I shall probably discuss the subject more at large hereafter; when furnished with a greater number of experiments I shall be able to speak about it with more confidence.

ARTICLE IV.

An Essay on the Shapes, Dimensions, and Positions of the Spaces, in the Earth which are called Rents, and the Arrangement of the Matter in them. By Mr. John B. Longmire.

(*Continued from* p. 92.)

The Arrangement of the Matter in Bended-Tabular Rents.

THE most of bended-tabular rents contain matter which is similar to that of the formation in which they exist; but some of them contain matter very different to it. Hence in treating of the matter in these rents, a very natural arrangement is to divide them into such as contain matter which proceeded from that contiguous to them, and such as have been filled with matter that entered them at the earth's surface. Again, as the matter in all bended-tabular rents which contain matter similar to that of the formation in which they exist is arranged in the shape of tabular masses accompanied with crystals; and sometimes with variously shaped masses confusedly mixed together; and as all the rents which contain tabular masses contain *earthy* tabular masses, but only some of them *metallic* tabular masses, I divide them into those which contain earthy, and into those which contain earthy and metallic tabular masses.

FIRST DIVISION.

The Arrangement of that Matter in Bended-Tabular Rents which has proceeded from the Matter on the Sides of these Rents.

1. *Of the Matter in Rents containing only Earthy-Tabular Masses.*

In bended-tabular rents filled with earthy matter only, but which has proceeded from the matter of the formation in which these rents exist, two sorts of earthy-tabular masses are found. The masses of one sort vary in denomination as do the strata; but those of the other are generally of one denomination throughout a rent, and their arrangement is very different from that of the first sort. These masses I divide, for reasons which will afterwards appear, into the first-formed and second-formed tabular masses.

Of the First-Formed Tabular Masses.

The first-formed tabular masses are known by the names of vein-stones, ribs, walls, &c. In the parts of these rents where the first-formed tabular masses exist very regularly arranged, the following order is observed. If A A 1, fig. 1, Plate XXXII., be a stratum of coal, in the rent H I a tabular mass of coal G is so situated that its under side *m d* lies between two lines *d*, *m*, one parallel to and opposite the line which joins the surfaces of separation *b e* to the under side *e f*, and the other parallel to and opposite the line *l*, and its upper side *a i* is similarly disposed towards the lines *b*, *h*, as is its under side towards the lines *e*, *l*. If the stratum B B 1 be slate-clay, it has a tabular mass of slate-clay *n* in the rent, similarly situated with respect to it, as the mass G is to the stratum A A 1; the stratum C, and every other stratum in the rent above the stratum A, has a tabular mass arranged in the same manner as the masses G O; and the strata D, E, F, and all the strata below them, have tabular masses *h*, *q*, *r*, &c. which are similarly disposed. But all the masses differ as much from one another in external characters, as the strata from which they proceed differ from one another.

The arrangement of the first-formed tabular masses is seldom so regular as it is here represented: for sometimes there are many small tabular masses between the surfaces of separation of the strata, as *f e*, fig. 2, represents. These masses are sometimes at great distances from one another, in the horizontal direction of the rent, and very small; but as we proceed horizontally towards the centre of the masses, as in the direction *a b*, they increase in dimensions, and at the centre, as at *b*, there is one large mass, which perhaps fills the whole height, and a considerable distance horizontally. Beyond this mass they decrease in dimensions, and as the distance from it increases, the distance from one another increases also; till at last they become what miners aptly call "grimings." Beyond this place, as beyond *c*, we find no more of these masses for a considerable distance *c d*; they then begin again, increase and decrease in magnitude as before.

The arrangement of the first-formed masses, as just described, is very perfect in all small rents of this shape; but in the larger it is seldom perfect.

The first-formed tabular masses, as I said before, resemble the matter contiguous to the rents in which they are situated; so that in the coal formation they consist of alternations of coal, slate-clay, white sand-stone, &c.; in the red, and white sand-stone, of red and white sand-stone; in lime-stone, of lime-stone; and in granite, gneis, sienite, &c. of these rocks respectively.

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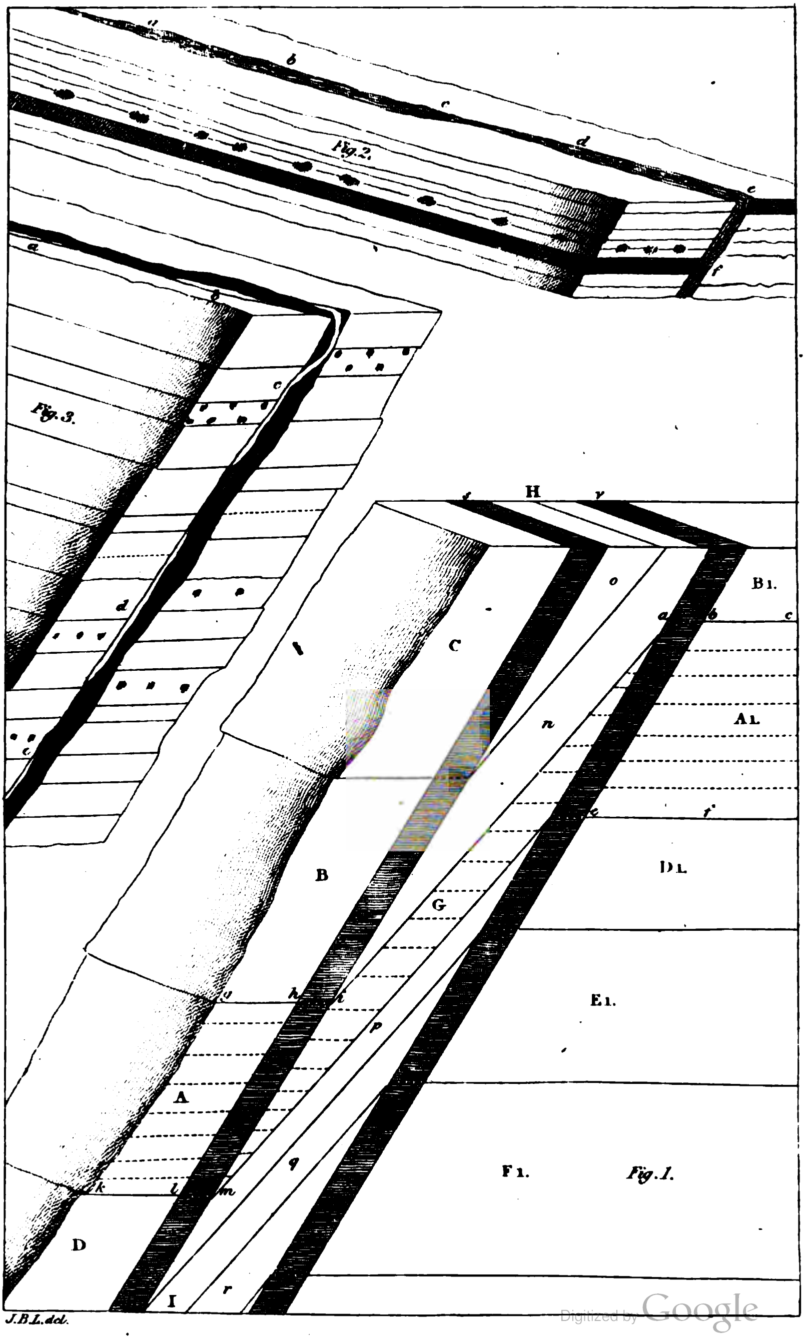

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Mr. Longmire on Rents. (Pl. II.)

When we see these tabular masses lying in a rent of the coal formation, between the surfaces of separation of the strata on the sides of that rent whose matter is the same as theirs; when in addition we recollect, as before stated, that these masses in granitic rents are granite, &c. we are irresistibly led to the conclusion that these masses are a part of the matter which existed in the formations before these rents took place; because had they proceeded either from above or below, they could not have assumed their present "*highly inclined positions*;" nor is it probable that they would be similar to the matter near the rents; and it is still less probable that where the matter of a formation consists of strata of different denominations, these masses would lie close to strata whose denominations are similar to theirs. These remarks are sufficiently conclusive: but there is one fact still which gives them greater weight, and that is, that parts of the coal from which these masses are supposed to proceed, are wanting to such distances from these rents, that the area of the parts wanted is about equal to that of the large masses in the rents. Therefore I think the position, that the matter thus arranged in rents proceeds from the matter which is contiguous to them, is fully proved.

The process by which these masses have acquired their present situations and positions may have been similar to that which is described as follows. After the formation of the separated surfaces of the stratum in which the lowest extremity of a rent is situated, the parts of that stratum which were still in a fluid state passed naturally into the rent, or were forced into it by the incumbent weight as the rent gradually increased in width. Several parts of the strata close to the rent would be fluid, therefore near the whole of it was filled opposite these parts; in other parts only small portions of the strata were fluid, of course only small parts entered the rent; and in other parts the strata were sufficiently solid to resist the weight of the incumbent matter, so that from these parts no matter was forced into the rent. It is easy to conceive that such a modification in the arrangement of the masses as that already described would take place in this manner. From the angular position of the masses in a bended-tabular rent, and from the other phenomena already described respecting them, I deduce the formation of the rent in the stratum opposite its lowest extremity so long before its commencement in the second stratum as to allow the matter which entered the rent from the lowest stratum to be so much consolidated, that it would retain its position as the rent continued to widen, and would support the fluid matter which rested on it when the formation of the rent in the second stratum from the bottom commenced; and the commencement of the rent's formation in every stratum, reckoning upwards, before its commencement in that stratum which rests on it, such a length of time as I have mentioned to take place between the commencement of the rent's formation in the lowest stratum, and of its commencement in that stratum which lies on it.

The foregoing description of the first-formed tabular masses, and their mode of arrangement, is of a general application: but many modifying circumstances are to be taken into the question in giving an account of all the variations in their arrangement; but a description of them would be too long for this essay.

Of the Second-Formed Earthy-Tabular Masses.

The second-formed tabular masses are known by the names of sticking, dowk, flookans, &c. These masses always resemble the softer parts of the matter contiguous to the bended-tabular rents; and consist chiefly of that part of it which is the easiest suspended mechanically in water. They, therefore, generally contain a large portion of clay. In the coal formation they resemble slate-clay, alum-slate, &c.; but in general they are much softer, and sometimes have the consistence of common clay. I have never met with them in lime-stone but in Derbyshire, and there they have proceeded from the slate-clay which lies on the lime-stone. In granite, gneis, and sienite, they consist of soft, greyish, greenish, or yellowish-white clayey matter, just as the colour of the felspar of these rocks may vary. In serpentine they sometimes constitute the substance called soap-rock. In Cornwall they are called flookans; and Price describes a flookan, in his Mineralogia Cornubiensis, "as a tenacious and glutinous earth or clay, that sometimes runs withoutside some veins, immediately between either wall of the lode and the lode itself, and more frequently adhering to the hanging or superior wall, and sometimes mixed in and throughout the lode itself."

The second-formed tabular masses are generally situated between the upper side of the rent and the first-formed tabular masses, and sometimes between these masses and the lower side of the rent. They lie parallel and close to the sides, and never stretch across the rent, like the first-formed tabular masses, except when they change from one side to another, which is very seldom. The unshaded parts *a*, *b*, *c*, *d*, *e*, fig. 3, represent these masses. They do not continue the whole length of a rent, but are frequently interrupted by the first-formed masses, and by the closing of the sides of a rent. Hence the spaces in the rents which contain these masses are of tabular shapes, which are of various figures and dimensions. These spaces are generally connected together by channels, which are sometimes very small. But although the greatest part of the matter is arranged in tabular masses which lie near the sides of these rents, yet the rest has adapted itself to every inequality in the surfaces of the first-formed tabular masses, and to those in the sides of the rents; and has also completely filled every cavity in the first-formed masses, and in the matter contiguous to the rents, to which they could gain access.

As the nature and position of the second-formed are so different to those of the first-formed tabular masses, these masses could not both acquire their situations at the same time; but as the former have adapted themselves to the inequalities of the latter, those must have been in a fluid state when these were solid. Hence the first obtained their situations after the second sort of masses. With the help of these observations, it is easy to account for the forming cause of the second-formed tabular masses. As the bended tabular rent continued to increase in height and width, fluid matter entered it, and the first-formed tabular masses were produced in the manner already described; but after a certain length of time, no more matter was separated from that contiguous to the rent; yet by the contraction of this matter, the rent continued to widen, and the first-formed masses contracted also, so that hollow spaces were formed in it. These spaces could not lie without some interruptions as to their existence opposite the whole of the superficial area of a rent; because some of the first-formed masses must lie against the sides of the rent, to retain their situations, and to support others which did not rest against the sides. The spaces so formed were gradually and slowly filled up in the *manner of a clayey sediment*, and thus produced the second-formed tabular masses.

2. *Of the Matter in Rents containing Earthy and Metallic-Tabular Masses.*

The arrangement of the earthy-tabular masses in rents which contain metallic-tabular masses is the same in every respect as in rents filled only with earthy-tabular masses, with the addition of being associated with the metallic masses which are found sometimes on the sides and middle of the rent, and which are subject to the same variation of thickness, and want of continuity, as the earthy-tabular masses. There is one circumstance in the arrangement of metallic matter which is peculiar to formations consisting of alternating strata of different denominations. The metallic masses are not found in rents opposite strata of every denomination, but only opposite one or two. Thus, in the mining district round Alston, in Cumberland, the strata consist of white sand-stone, lime-stone, coal, slate-clay, &c.; but the lead ore is only found in lime-stone and sand-stone, and never opposite the rest; except when the first-formed masses of lime-stone or sand-stone are situated in a rent, a little lower than the strata from which they proceed; then they sometimes have masses of lead ore adhering to them; and when the lead ore is found in small masses mixed throughout similar masses of lime-stone, sand-stone, and clay-slate, it is sometimes lying opposite the clay-slate strata; but here it is evident, from the lime-stone and sand-stone masses which accompany it, that it has fallen from opposite the lime-stone and sand-stone strata. The same peculiarity takes place in the Derbyshire mines. There the lead ore is found only opposite the lime-stone, and not opposite the amygdaloid, or the slate-clay strata, except when accompanied with lime-stone masses.

As there are no metallic strata opposite most of the bended-tabular rents from which the metallic matter could proceed, and as the arrangement of this matter in these rents is such as to show most distinctly that it has not been deposited from a solvent which might once cover the earth, nor has been forced into its present situation from below in a fluid state; so its existence in these rents embarrasses very much the speculator on the source of the matter in them. But as the metallic masses are associated with the earthy, which I have shown to proceed from the contiguous matter, I would be very much inclined to suppose that the metallic matter has the same source. But although the mechanical part of the process admits of proof, yet the chemical part is difficult to reconcile with the present principles of chemistry. Still, however, from my knowledge of the mutual arrangements of the earthy and metallic-tabular masses, I will venture to hazard the conjecture, that some profound chemist will discover much nearer relations between the internal properties of metallic and earthy matter than we are at present aware of.

SECOND DIVISION.

The Arrangement of that Matter in Bended-Tabular Rents which entered them at the Earth's Surface.

Although the matter in the most of bended-tabular rents is similar to, and has proceeded from, that in which they are situated, yet into some of them it has entered from the earth's surface, in either a fluid or a solid state.

The matter which entered these rents from above in a fluid state is green-stone, basalt, porphyry-slate, &c. Many of the rents so filled contain throughout matter of one denomination; but a few of them of two or three denominations. Rents filled from above with the matter of green-stone, basalt, &c. are frequently found in the coal, red, and white sand-stone formations, and are abundant in many parts of Scotland, in these and some other formations.

When the rents so filled were formed, the matter in which they are situated was so near its present state of solidity that no matter from that source could be forced into them by the incumbent weight. They, therefore, remained empty, till fluid substances, by a part of which they are filled, travelled over them to take their situations in lower parts of the same formations, or in others which are on lower levels; and in doing so, matter entered, and then filled them to the earth's surface.

Bended-tabular rents, filled with matter which entered them in a solid state, contain clay, sand, gravel, and stones, either separate or mixed together. A few of them are found in almost every formation of coal. Such rents were formed at the same time as those containing green-stone, &c.; but as these fluid substances did not travel over them, they remained empty till filled by the process which produced the alluvial matter, by a part of which they are filled.

Of the Straight-Tabular Rent.

This rent is straight in both directions. Hence its name. Its position is always at right angles to that of the strata. Hence it is generally either perpendicular, or angular with a great ratio of angularity, or approaching nearly to a perpendicular line. There is no alteration in the position of the strata contiguous to it, as any stratum on one side is opposite its corresponding part on the other. This is a circumstance that at first sight distinguishes it from the bended-tabular rent. It is filled with matter that entered it from above; such as the matter of green-stone, basalt, &c. or clay, stones, sand, and gravel.

This rent was formed when the earth's matter was so near its present state of solidity, that the *inequality* in its contraction was so very small as not to be discernible. Hence we find the strata on the same level on both sides of it.

The cylindrical and ovalar rents I will not at present describe, as they are not very important phenomena in a geognostic point of view. They are very well described in William's Mineral Kingdom, under the names of pipe and flat veins.

In my next communication I will describe the *junctions* of tabular rents one with another.

Errata in Mr. Longmire's first Communication on Rents.

Read as follows the sentence which begins on the 42d line of the 84th page:—"And Professor Jameson mentions two near Freyberg, in Germany; one of which is more than four miles in its *horizontal direction*, and the other more than ten miles in this direction."

The letter *w* in fig. 1, Plate XXIX., ought to have been placed close to the highest extremity of the rent, *v*, *w*, *h*.

In fig. 3 of the same plate some letters are misplaced: these, however, will be best corrected by repeating the figure in a future plate.

Article V.

Experiments upon Green Uran Mica, with a view to its Chemical Analysis. By the Rev. W. Gregor.

Any general account of the external character and appearance of a mineral that is so well known would be superfluous. A *scientific* description, that can be useful or interesting, must be left to the experienced crystallographer.

The subject of the following observations was raised in the copper-mine called Gunnis Lake, in the eastern extremity of the county of Cornwall.

The crystalline laminæ are, in general, very free from extraneous substances. Small fragments of quartz, however, and a light ochrey matter, easily separable by washing, are sometimes found adhering in small quantities to them.

The purest pieces were carefully selected for experiment.

The specific gravity of this fossil, taken at the temp. 52 of Fahr. was 3·8.

A portion of it, reduced to a fine powder, was exposed, in a small glass retort, to an open fire. A dew soon collected on the neck of the retort, and passed into the receiver. It was pure water.

100 grains, that had been exposed to a low red heat for ten minutes in a platinum crucible, weighed whilst still warm = 84·6. The mineral, therefore, had lost 15·4 per cent. Another portion exposed to a stronger heat for one hour was diminished, in respect of weight, very nearly in the same proportion.

The mineral, after ignition had lost its brilliancy, and had assumed the appearance of fine brass filings. 100 grains, which had not been ignited, in the state of a fine powder, were put into a matrass, and covered with pure nitric acid, moderately diluted with water. The acid produced no effervescence, and whilst it was unaided by heat, it seemed to act very feebly upon the mineral for many hours. The vessel was then placed in a digesting heat. Yet the solution proceeded very slowly, and was frequently interrupted by the deposition of a green crust, which from time to time covered the bottom of the vessel, and protected the mass from the action of the acid. Boiling water produced little or no change on this crust. It became necessary, therefore, repeatedly to add fresh portions of nitric acid in order to dissolve it. At last, by means of an excess of acid, a solution of the mineral was effected, except a very small quantity of residuary matter, which after ignition = 0·1 gr. It was a mixture of quartz and oxide of iron; and as it was extraneous, I dissolved an equal quantity (0·1 gr.) of pure uran mica in nitric acid, and added the solution to the former one.

The solution was light green. I now poured into it ammonia in excess, which separated a yellowish-white matter. The blue supernatant fluid was carefully poured off from the subsided precipitate, and fresh portions of ammonia were repeatedly supplied, and the matrass containing it was from time to time shaken, and at last exposed to a digesting heat.

When the last effusions of ammonia, aided by the heat of the sand-bath, showed not the slightest indications of copper, but were poured off absolutely colourless,* the undissolved matter was mixed with distilled water, and thrown upon a filter, and sufficiently edulcorated. What was thus separated by the filter was gradually dried, collected, and ignited. It was weighed whilst *still warm*, and it amounted to 74·9 grains. It was of a light yellow colour (A). This I had reason to consider as oxide of uranium; but subsequent experiments proved that this amount must be subjected to some slight corrections.

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* When in a former analysis I operated upon the same quantity of uran mica which *had been ignited*, I found that the last portions of ammonia, when they had ceased to be tinged blue, became of an opal hue, and that a considerable quantity of distilled water passed through the filter before it became perfectly transparent. In this case nearly two per cent. of uran oxide were suspended by the ammonia.

II. The ammoniacal solution of copper, with the water of edulcoration, was evaporated gradually to dryness;* and with a view of ascertaining whether it contained any of the oxide of uranium, I redissolved the mass, which remained, in nitric acid, and treated the solution, as before, with ammonia. A very small quantity of yellowish-white matter remained undissolved, which after ignition = 0·2. It was oxide of uranium, and must therefore be added to 74·9.

The ammoniacal solution of copper was now evaporated to dryness, again dissolved in nitric acid, and boiled with liquid potash, which precipitated the copper in the state of oxide, which after ignition = 7·65.

III. The 74·9 gr. of supposed oxide of uranium were moistened with distilled water, and some *test sulphuric acid* was gradually poured upon the mass, and the vessel containing it was placed in warm sand. The mass soon became dark green, and resembled in consistency, as well as in intensity of colour, the pigment called sap-green, when suspended in water. What the acid had not dissolved appeared gelatinous; and when warm water was poured upon it, and it was stirred with a glass rod, it separated into lumps somewhat resembling spermaceti. They were encompassed by a greenish fluid, and required repeated effusions of sulphuric acid for their solution. A greenish-white matter resisted the acid. It was carefully collected, dried, and ignited; it = 0·6, treated with carbonate of soda, in a platinum crucible, and subsequently with nitric acid, &c. It proved to consist chiefly of oxide of uranium, with a very small portion of oxide of lead, too small, indeed, to admit of being accurately weighed.† Upon dropping ammonia into the solution of the oxide of uranium, which I had effected by means of sulphuric acid, I was surprised to find that it was tinged blue, indicating thereby the presence of copper. The same process with ammonia, before detailed, was repeated, except that at last the copper was dissolved in sulphuric acid, and separated in a metallic state by a cylinder of zinc. The copper thus obtained, after having been edulcorated, and shaken with very dilute muriatic acid, and carefully dried, weighed 0·5, which, with the accession of oxygen in the ratio of 25 per cent., is equivalent to 0 62 of oxide. The 74·9 grains of supposed uran oxide must therefore be diminished by the subtraction of 0·62 of oxide of copper = 74·28, which, with the accession of 0·2 of uran oxide, that was mixed with the copper in the ammoniacal solution, will constitute the whole amount of uran oxide 74·48. The 7·65 grs. of copper must be increased by 0·62 = 8·27.

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* In a former experiment upon 100 gr. of this mineral I evaporated the cupreous ammoniac to dryness, and exposed the dry mass to a red heat, but I could not detect the slightest trace either of potash or soda.

† In my former analysis I detected a small quantity of oxide of lead also.

After the last separation of copper, I could not by any methods discover any substance in the residuum but oxide of uranium. The relative amount of the several ingredients of uran mica stated, at one place of decimals, will be as follows :—

Oxide of uranium, with a trace of oxide of lead..	74·4
Oxide of copper	8·2
Water	15·4
Loss	2
	100

This statement agrees so very nearly with the result of a former analysis of the same quantity of uran mica, that I do not think it necessary to set down the average mean of the two operations.

The copper exists in this fossil in a much larger proportion than it was conceived to be.

I should not, probably, lay much stress upon the minute quantity of oxide of lead which I found in this fossil, if I had not recollected that I had some years ago detected the same oxide as more decidedly constituting an ingredient of the *yellow uran mica.*

The ease with which this fossil parts with its water in a low red heat is a circumstance that will probably prevent its being denominated a hydrate of uranium and copper.

Creed, Feb. 27, 1815. W. Gregor.

Article VI.

Register of the Weather in Plymouth for the last Six Months of 1814. By James Fox, jun. Esq. With a Plate.

JULY.

Date.	Wind.	Rain.	Observations.
1814.			
July 1	Var.	0·01	Light rain, morn; fair day.
2	Ditto	—	Fair.
3	Ditto	—	Ditto.
4	Ditto	—	Cloudy morn; fair day.
5	Ditto	—	Fair.
6	Ditto	—	Ditto.
7	W S W	0·15	Cloudy; light rain.
8	S S W		Light rain, morn; cloudy day.
9	Ditto		Cloudy.
10	S		Fair.

Date.	Wind.	Rain.	Observations.
1814.			
July 11	W to N W		Fair.
12	Var.		Ditto.
13	N W		Ditto day; cloudy at night.
14	Ditto	1·02	Cloudy day; light rain at night.
15	S W to S E		Cloudy morn; heavy rain, afternoon.
16	N W		Cloudy morn; fair eve.
17	S S W		Fair morn; misty eve.
18	N W		Cloudy and fair; ditto, eve.
19	S to W	0·36	Fair morn; heavy showers, eve.
20	S S W to N W		Fair.
21	W S W	0·01	Fair morn; misty eve.
22	Ditto		Fair.
23	S to E		Ditto.
24	E		Ditto; high wind.
25	S		Cloudy and fair; sultry.
26	S to E	0·43	Ditto; light rain, morn.
27	E N E to E		Ditto, ditto.
28	E S E		Heavy rain, morn; cloudy eve.
29	S W to W N	0·05	High wind; shower, morn; fair noon; misty eve.
30	N N W		Cloudy and fair.
31	S to N E		Fair day; cloudy at night.
		2·03	Inches rain.

			Wind.
Barometer:	Greatest height	30·22 inches	E
	Lowest	29·60	N W
	Mean	29·917	
Thermometer:	Greatest height	76°	E
	Lowest	47	Var.
	Mean	63·31	

AUGUST.

Aug. 1	W N W to W		Cloudy and fair.
2	S		Fair.
3	W N W		Misty morn; fair day.
4	S	0·19	Fog, morn; fair day; light rain, night.
5	W S W		Showers, and high wind.
6	W N W	0·76	High wind; heavy ditto morn; cloudy and fair day.
7	S W		Ditto; cloudy morn; showers, eve.
8	W N W		Ditto; showery day.
9	Ditto	0·12	Ditto; cloudy morn; showers, eve.
10	N W		Showers, morn; cloudy eve.
11	S W		Cloudy and fair.
12	W S W		Misty morn; fair day.
13	S W to N W	0·20	Light rain, morn; cloudy and fair.
14	W S W to N W	0·21	Ditto, morn; fair afternoon.
15	W S W		Showers.
16	N W	0·20	Cloudy and fair.
17	W S W		Misty; light rain.
18	W N W		Ditto, morn; cloudy and fair eve.
19	N W	0·09	Cloudy morn; showers, noon; fair eve.
20	S W to S	0·36	Showery day; high wind, eve.
21	S to W N W	0·05	Fog, morn; cloudy and light rain.

Google

Date.	Wind.	Rain.	Observations.
1814.			
Aug. 22	E N E to S E	0·01	Cloudy, and a shower.
23	S to S W	0·49	Heavy rain, morn; fair afternoon.
24	S	0·06	Shower, morn; ditto, ditto.
25	W N W	0·09	Cloudy, and showers.
26	N N W		Ditto, and fair.
27	Ditto		Ditto, ditto.
28	Var.		Ditto, ditto.
29	Ditto		Fair.
30	Ditto	0·01	Cloudy morn; fair afternoon; shower at eve.
31	Ditto		Fog, morn: fair day.
		2·84	Inches rain.

			Wind.
Barometer:	Greatest height................	30·29 inches	Var.
	Lowest	29·64	S
	Mean	29·993	
Thermometer:	Greatest height................	76°	W
	Lowest	46	S W
	Mean	61·8	

SEPTEMBER.

Sept. 1	E		Fair.
2	E N E		Ditto.
3	Ditto		Ditto.
4	Ditto		Ditto.
5	Var.		Fog, morn; fair day.
6	W N W		Ditto, ditto; cloudy at night.
7	N W	0·10	Showers.
8	E N E to S		Cloudy and fair.
9	Var.		Fair.
10	Ditto		Ditto; *very dark* clouds in the E. and S. E. at night.
11	E N E		Fair.
12	Var.		Misty morn; fair afternoon.
13	N E		Fair.
14	E		Ditto.
15	E		Ditto; high wind.
16	E		Ditto.
17	E		Ditto.
18	S to E N E		Ditto, morn; cloudy eve.
19	S S E		Fair.
20	S	0·10	Cloudy, and a shower.
21	S S W to W S W		Fair morn; showers, afternoon.
22	W S W		Cloudy and fair.
23	S W to S S W	0·23	Heavy showers.
24	S S E to N W	0·92	Heavy rain, morn; showery day.
25	S S W		Fair day; cloudy at night.
26	S S E	0·57	A gale of wind, and heavy rain, morn; hail and thunder storm at night.
27	S W to S E	0·05	A shower, morn; fair day.
28	Var.	0·02	Misty morn; fair day.
29	E N E		High wind; cloudy.
30	Ditto		Ditto, ditto; ditto.
		1·99	Inch rain.

			Wind.
Barometer:	Greatest height	30·32 inches	E
	Lowest	29·53	N W
	Mean	30·02	
Thermometer:	Greatest height	74°	S S E
	Lowest	41	N E
	Mean	59·2	

OCTOBER.

Date.	Wind.	Rain.	Observations.
1814.			
Oct. 1	E N E		Very high wind; cloudy and fair.
2	Ditto		High wind; ditto.
3	Ditto		Ditto; fair.
4	E		Fair.
5	Ditto		Fog, morn; fair day.
6	E to N N W		Cloudy and fair.
7	N W to N E		Fog, morn; fair day.
8	Var.		Fair.
9	N E		Ditto.
10	E to S		Hoar frost, fog, and fair morn; cloudy afternoon.
11	S S W		High wind; cloudy.
12	S to E S E	0·21	Ditto; showery.
13	S E	0·37	Ditto; showers at night.
14	E S E to W N W		Ditto; showery day.
15	W to S	0·40	Fair morn; high wind, and showers, afternoon.
16	W N W		Fair morn; cloudy afternoon.
17	S	0·28	Showery morn; heavy rain at night.
18	S S W to Var.	0·38	High wind, and very heavy showers.
19	N W	0·28	Ditto, ditto, and hail ditto.
20	W N W		Ditto; fair morn; cloudy eve.
21	Var.		Fog, morn; fair day; cloudy at night.
22	W N W		Fog, ditto; showery eve.
23	N W	0·31	High wind; showery.
24	E N E to S E	0·42	Misty day; a storm, and heavy rain, at night.
25	W N W	0·48	Heavy showers, with hail.
26	E N E		Fog, and fair morn; cloudy eve.
27	Var.		Ditto, ditto, ditto.
28	Ditto	0·30	Showery day.
29	Ditto	0·32	Ditto.
30	E S E		Ditto.
31	E	0·22	Ditto; high wind.
		3·97	Inches rain.

			Wind.
Barometer:	Greatest height	30·21 inches	E
	Lowest	29·07	N W
	Mean	29·771	
Thermometer:	Greatest height	64°	Var.
	Lowest	31	E
	Mean	49·854	

NOVEMBER.

Date.	Wind.	Rain.	Observations.
1814.			
Nov. 1	E N E		High wind; cloudy.
2	Ditto		Ditto, ditto.
3	Ditto		Ditto, ditto.
4	Ditto		Ditto; cloudy and fair.
5	Var.		Cloudy.
6	N W to N		Hoar frost; fog, morn; fair day.
7	E N E to N W	0·18	Cloudy; showers at night.
8	N W		High wind; showery day.
9	N W		Cloudy.
10	N N E to E		Hoar frost; fair.
11	W to S W	0·05	Ditto; misty morn; storm at night.
12	W N W	0·17	High wind, and showers.
13	W N W		Showers.
14	Ditto	0·05	Cloudy day; light rain, eve.
15	S W		Misty day.
16	W N W	0·35	Heavy showers; fair eve.
17	W S W		Misty day.
18	S S W to N W	0·31	Fair morning; showery afternoon.
19	N W		Fog, ditto; ditto.
20	N W to N N E		Cloudy and fair day; fair at night.
21	N W to N E		Hoar frost; fog, morn; fair day.
22	N E to S S E	0·18	Ditto; high wind and showers.
23	S E to E N E		Cloudy.
24	S to S W	1·02	Ditto, morn; heavy rain, afternoon.
25	S W to W		Misty morn; showers, afternoon.
26	N W	0·17	High wind, and showers.
27	E to N W	0·14	Ditto, ditto.
28	Var.	0·37	Thick weather, morn; heavy rain, afternoon.
29	W to W N W		Ditto, and high wind.
30	N W	0·16	Showers.
		3·15	Inches rain.

			Wind.
Barometer:	Greatest height	30·36 inches	E
	Lowest	29·23	N W
	Mean	29·771	
Thermometer:	Greatest height	54°	S S W
	Lowest	24	N E
	Mean	43·3	

DECEMBER.

Dec. 1	N W	0·05	Fair morn; showery afternoon.
2	E N E		Hoar frost; fog, morn; cloudy day.
3	E N E to S S E		Ditto, ditto; cloudy and fair day; high wind at night.
4	W N W to N W	0·67	Heavy showers.
5	N W	0·08	Showers.
6	E N E to S		Hoar frost; fair morn; cloudy afternoon.
7	N W to W	0·61	High wind, and heavy showers.
8	S to S W	0·40	Ditto; a wet day.

Date.	Wind.	Rain.	Observations.
1814.			
Dec. 9	W N W to E N E	0·51	Showers.
10	S E to W		A wet day; high wind.
11	W to S W	0·31	Ditto.
12	S S W to S W	0·35	Ditto, and *violent* storm.
13	S W to W S W	0·42	A storm; showery.
14	S W		Ditto, ditto.
15	S W	0·25	Ditto, ditto.
16	S W	0·28	A violent ditto, ditto.
17	S S W to S W		A storm, ditto.
18	S W to S S W		A high wind; thick weather.
19	S W to N W	0·41	Showers.
20	E		Thick weather, and showers, morn; cloudy afternoon.
21	E S E	1·06	A storm, showers, and sleet.
22	Ditto to N E		Heavy rain; fair at night.
23	E		Showers, morn; cloudy day.
24	E N E	0·17	Cloudy, with light rain.
25	N N E		Hoar frost; cloudy day.
26	N N W to E S E		Ditto, ditto; showers of sleet.
27	Var. at S.	0·60	*Heavy* showers.
28	Ditto at N		Cloudy day.
29	Ditto	0·32	Steady rain, morn; misty eve.
30	S to N W	0·41	Showers, morn; fair eve.
31	N W to S E		Cloudy and fair day.
		6·70	Inches rain.

			Wind.
Barometer:	Greatest height	30·19 inches	S E
	Lowest	28·96	Var.
	Mean	29·688	
Thermometer:	Greatest height	54°	S W
	Lowest	28	N N W
	Mean	43·677	

ARTICLE VII.

Magnetical and Astronomical Observations at Hackney Wick.

By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North. Longitude West in Time $6''\frac{82}{100}$.

March 14, Immersion of ξ Ceti..	7h	9′	15·1″	Mean Time at Hackney Wick.
Mar. 14, Immersion of Jupiter's 2d Satellite	9	50	36·6	Ditto at Hackney Wick.
	9	50	43·4	Ditto at Greenwich.

Magnetical Observations.

1815.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Feb. 18	8h 25′	24° 15′ 02″	1h 35′	24° 22′ 09″	Not observed.	Not observed.
Ditto 19	8 25	24 14 14	1 35	24 22 42		
Ditto 20	8 25	24 15 03	1 35	24 21 24		
Ditto 21	— —	— — —	1 25	24 22 25		
Ditto 22	8 30	24 14 56	1 25	24 20 01		
Ditto 23	8 25	24 14 32	— —	— — —		
Ditto 24	8 45	24 14 16	1 20	24 20 43		
Ditto 25	8 40	24 13 35	1 30	24 20 29		
Ditto 26	8 45	24 15 32	1 25	24 22 01		
Ditto 27	8 35	24 14 51	1 20	24 23 10		
Ditto 28	8 30	24 15 38	1 30	24 23 28		

1815. Mean of Observations in Feb.	Morning	at	8h	37′.....	Variation	24°	15′	11″	West.
	Noon	at	1	31	Ditto	24	21	51	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	47	Ditto	24	16	46	West.
	Noon	at	1	36	Ditto	24	20	12	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
1814. Ditto in Dec.	Morning	at	8	44	Ditto	24	18	02	West.
	Noon	at	1	30	Ditto	24	20	36	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	41	Ditto	24	16	20	West.
	Noon	at	1	40	Ditto	24	20	37	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	39	Ditto	24	14	08	West.
	Noon	at	1	42	Ditto	24	21	45	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	32	Ditto	24	14	33	West.
	Noon	at	1	39	Ditto	24	23	17	
	Evening	at	6	19	Ditto	24	16	50	
Ditto in Aug.	Morning	at	8	30	Ditto	24	14	13	West.
	Noon	at	1	39	Ditto	24	23	48	
	Evening	at	6	57	Ditto	24	16	31	
Ditto in July.	Morning	at	8	41	Ditto	24	13	29	West.
	Noon	at	1	42	Ditto	24	23	44	
	Evening	at	6	58	Ditto	24	17	00	
Ditto in June.	Morning	at	8	44	Ditto	24	13	10	West.
	Noon	at	1	39	Ditto	24	22	48	
	Evening	at	6	52	Ditto	24	16	29	
Ditto in May.	Morning	at	8	45	Ditto	24	13	12	West.
	Noon	at	1	44	Ditto	24	22	13	
	Evening	at	6	38	Ditto	24	16	14	
Ditto in April.	Morning	at	8	45	Ditto	24	12	53	West.
	Noon	at	1	48	Ditto	24	23	53	
	Evening	at	6	29	Ditto	24	15	30	
Ditto in March.	Morning	at	8	52	Ditto	24	14	29	West.
	Noon	at	1	52	Ditto	24	23	08	
	Evening	at	6	11	Ditto	24	15	33	
Ditto in Feb.	Morning	at	8	47	Ditto	24	14	50	West.
	Noon	at	1	52	Ditto	24	20	58	
	Evening	at	—	—	Ditto	—	—	—	Not obs.

Mean of Observations in Jan.	Morning	at	8h	52′	.....	Variation	24°	15′	05″	West.
	Noon	at	1	53	.....	Ditto	24	19	03	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at	8	53	.....	Ditto	24	17	39	West.
	Noon	at	1	51	.....	Ditto	24	20	30	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	42	.....	Ditto	24	17	17	West.
	Noon	at	1	54	.....	Ditto	24	20	24	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	.....	Ditto	24	15	41	West.
	Noon	at	1	59	.....	Ditto	24	22	53	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	.....	Ditto	24	15	46	West.
	Noon	at	2	02	.....	Ditto	24	22	32	
	Evening	at	6	03	.....	Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	.....	Ditto	24	15	55	West.
	Noon	at	2	02	.....	Ditto	24	23	32	
	Evening	at	7	05	.....	Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	.....	Ditto	24	14	32	West.
	Noon	at	1	50	.....	Ditto	24	23	04	
	Evening	at	7	08	.....	Ditto	24	16	43	
Ditto in June.	Morning	at	8	30	.....	Ditto	24	12	55	West.
	Noon	at	1	33	.....	Ditto	24	22	17	
	Evening	at	7	04	.....	Ditto	24	16	04	
Ditto in May.	Morning	at	8	32	.....	Ditto	24	12	02	West.
	Noon	at	1	37	.....	Ditto	24	20	54	
	Evening	at	6	14	.....	Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	.....	Ditto	24	09	18	West.
	Noon	at	0	59	.....	Ditto	24	21	12	
	Evening	at	5	46	.....	Ditto	24	15	25	

Magnetical Observations continued.

1815.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
March 1	8h	20′	24°	15′	38″	1h	30′	24°	23′	32″	Not observed.	Not observed.
Ditto 2	8	35	24	14	45	1	45	24	20	08		
Ditto 3	—	—	—	—	—	1	40	24	22	06		
Ditto 4	8	35	24	14	43	1	50	24	22	20		
Ditto 5	8	35	24	13	17	1	20	24	25	32		
Ditto 6	8	35	24	16	51	1	35	24	24	41		
Ditto 7	8	40	24	14	47	1	25	24	22	26		
Ditto 9	8	25	24	18	25	1	15	24	23	06		
Ditto 10	8	35	24	15	43	1	15	24	24	17		
Ditto 11	8	25	24	15	51	1	30	24	21	55		
Ditto 12	8	45	24	18	32	1	30	24	22	48		
Ditto 13	8	40	24	14	22	1	20	24	20	35		
Ditto 14	8	35	24	13	33	1	25	24	22	28		
Ditto 15	—	—	—	—	—	1	25	24	25	16		
Ditto 16	8	40	24	14	12	1	25	24	25	52		
Ditto 17	8	30	24	16	47	1	30	24	23	01		

Feb. 20.—The wind blew very hard and squally from the west, with hard showers; and the needle vibrated at intervals 28′ 30″; and, what is remarkable, the needle, comparatively speaking, has been steady to the present day, March 17.

Rain fallen { Between noon of the 1st Feb. / Between noon of the 1st Mar. } 0·330 inches.

Comparison of the Variations in the Years 1813, 1814, *and* 1815.

		1813.			1814 and 1815.			Difference.		
April	Morning	24°	09′	18″	24°	12′	53″	+	3′	35″
	Noon	24	21	12	24	23	53	+	2	41
	Evening	24	15	25	24	15	30	+	0	05
May	Morning	24	12	02	24	13	12	+	1	10
	Noon	24	20	54	24	22	13	+	1	19
	Evening	24	13	47	24	16	14	+	2	27
June	Morning	24	12	35	24	13	10	+	0	35
	Noon	24	22	17	24	22	48	+	0	31
	Evening	24	16	04	24	16	29	+	0	25
July	Morning	24	14	32	24	13	29	−	1	03
	Noon	24	23	04	24	23	44	+	0	40
	Evening	24	16	43	24	17	.00	+	0	17
Aug.	Morning	24	15	55	24	14	13	−	1	42
	Noon	24	23	32	24	23	48	+	0	16
	Evening	24	16	08	24	16	31	+	0	23
Sept.	Morning	24	15	46	24	14	33	−	1	13
	Noon	24	22	32	24	23	17	+	0	45
	Evening	24	16	04	24	16	50	+	0	46
Oct.	Morning	24	15	41	24	14	08	−	1	33
	Noon	24	22	53	24	21	45	−	1	08
	Evening	—	—	—	—	—	—		—	—
Nov.	Morning	24	17	17	24	16	20	−	0	57
	Noon	24	20	24	24	20	37	+	0	13
	Evening	—	—	—	—	—	—		—	—
Dec.	Morning	24	17	39	24	18	02	+	0	23
	Noon	24	20	30	24	20	36	+	0	06
	Evening	—	—	—	—	—	—		—	—
Jan.	Morning	24	15	05	24	16	46	+	1	41
	Noon	24	19	03	24	20	12	+	1	09
	Evening	—	—	—	—	—	—		—	—
Feb.	Morning	24	14	50	24	15	11	+	0	21
	Noon	24	20	58	24	21	51	+	0	53
	Evening	—	—	—	—	—	—		—	—

Errata in Comparison of Variation.

May, morning observation, *for* 12′ 49″ *read* 13′ 12″; difference *for* + 0′ 47″ *read* + 1′ 10″.

November, noon observation, difference *for* − 0′ 13″ *read* + 0′ 13″.

January, morning observation, *for* 16′ 26″ *read* 16′ 46″.

Article VIII.

Reply to Mr. Hume's Answer to Mr. R. Phillips's Animadversions.[*]

(To Dr. Thomson.)

SIR,

Mr. Hume describes me as unfortunate in having noticed an error he had committed, because I was not aware that it had been previously detected and exposed by you. To be ignorant is unquestionably a misfortune; but he is surely less censurable who is unacquainted with one fact, stated by only one author, than he who, ignorant of many discoveries, related by several writers, denies them the credit of priority, and claims it for himself: the former is my case, the latter Mr. Hume's; when candour has done its utmost in allowing him the plea of ignorance.

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[*] *Annals*, p. 116 of the present volume.

To your decision, however, Mr. Hume says that he assented long ago; but so silent has been his admission of what he could no longer deny, that even his friends appear not to have known of this sacrifice of fame to truth: for Mr. Parkes, who long "has been even personally acquainted with" Mr. Hume, has admitted his claim to the discovery alluded to, in the last edition of the Chemical Catechism; a work deservedly popular, and with which Mr. Hume cannot be unacquainted.

I am "equally unfortunate," according to Mr. Hume, "in the two experiments quoted from M. Sage; and have drawn inferences diametrically opposite to those of all the chemists who have written on the subject, especially those of France." If my inferences are just, it matters not from whom I differ; and it would have been more to Mr. Hume's advantage, to show my error, than to overwhelm me with a pretended host of opposing authorities; I say pretended, for I confidently challenge him to produce the names of those chemists, who have drawn from M. Sage's experiments inferences diametrically opposite to mine. M. Sage says "Nitrous acid of 32 degrees occasions at first a brisk effervescence with aërated heavy spar; the nitre which results, requiring much water for its solution, precipitates as soon as it is formed." "(se precipite aussitôt qu'il se forme.)" Now the inference which I drew from this experiment, is "that nitrate of barytes is perfectly insoluble in nitrous acid of the usual specific gravity;" to copy the terms in which Mr. Hume has claimed priority of discovery, but omitting indeed the words in which he speaks of an insoluble solution; a thing much beyond my comprehension, and to the discovery of which I suppose he will be allowed an uncontested right.

The other experiment quoted from Sage, is, that "marine acid dissolves aërated heavy spar with effervescence, the salt which results precipitates immediately. The supernatant marine acid does not hold in solution (ne tient point en dissolution) any salt with a base of heavy earth." The fact expressed in the quotation with a precision that no sophistry can evade, and claimed by Mr. Hume as his own discovery, is "that muriate of barytes is virtually insoluble in muriatic acid."

This appears to me the plain statement of the controversy; but Mr. Hume has attempted to elude these deductions by a truly notable expedient; for it is one which, if it succeed in supplying his present purpose, must inevitably deprive him of the priority of the two discoveries remaining out of seven, which he has claimed in his paper on barytes, in 1802. Mr. Hume asserts that in Sage's experiments, "there is neither water of solution nor water of crystallization;" if this is the case, the acids he employed were concentrated acids. With this view of the subject, let us turn to Mr. Hume's paper in the Phil. Mag. for 1802, in which his fourth and sixth discoveries are thus described: "carbonate of barytes may be entirely changed into nitrate by nitrous acid in its concentrated state."—"Carbonates of barytes may be rendered into muriate by concentrated muriatic acid." Mr. Hume further contends, that there is no ground for asserting, that suffering the muriatic and nitric acids to form their respective salts from the decomposition of the carbonate "is precisely the same as adding their salts, or more particularly their solutions, to the respective acids;" he does not, however, attempt to show the difference; the facts of the case are, I assert, simply these: when crystals are added to the acid they remain undissolved; when a solution is substituted for them, it is decomposed, and crystals are deposited on account of their insolubility; and when water has been put to a strong acid, carbonate of barytes is decomposed, an aqueous solution is first formed in, and then decomposed by, the acid; crystals are formed, and remain undissolved; the manipulation in these cases differs, but the same substances are employed, and the same results produced; and these in every case (again to copy Mr. H.'s words) may be correctly described by stating "that such is the avidity of nitrous and muriatic acids for water, that they will attract even the whole of the water of solution from their respective salts."

Mr. Hume now says that when he published his paper on barytes in 1802, "it had been a common practice to ascertain the purity of nitrous acid, and even to purify it, by dropping into it a solution of nitrate of barytes." This statement, as well as some other parts of Mr. Hume's letter to you, Sir, would induce any one, unacquainted with his controversial style, to suppose that his alleged discoveries were attended with directions for avoiding so erroneous a practice. This, however, is entirely an after-thought; for the fact is, that Mr. Hume's paper on barytes does not contain any notice of the errors which the facts he mentions are likely to occasion; and consequently no "instructions" whatever are offered on the subject of purifying nitric acid. The experiments are related both by Mr. Hume, and by Sage who preceded him, without any reference to this subject; and nothing can be learned from the former which the latter does not supply.

According to the purpose to be answered Mr. Hume puts different constructions upon similar phrases. Thus, in the paper on barytes, the quotation "Tous les acides mineraux decomposent ce sel," meaning "carbonate de baryte," must be translated *diluted acids*; otherwise he must abandon a claim already quoted, but not very clearly made out, to the discovery that carbonate of barytes is decomposable by *concentrated* nitric and muriatic acids: whereas, in his letter, he attempts to show that Lagrange was ignorant that strong nitric acid decomposes solution of nitrate of barytes; and to prove this case, the words "acide nitrique," which he quotes from this author, must be rendered *concentrated nitric acid*. Thus Mr.

Hume, to substantiate his own discovery, makes acids *weak*, and to prove another's ignorance they become *strong*.

The fate of so large a proportion of Mr. Hume's claims to originality has naturally made him anxious for the remainder. Concerning the "silver test," he evinces painful forebodings, which are ill concealed by the seeming pleasantry with which they are accompanied. In one of his numerous communications respecting "this excellent test," he says; "the nitrate of silver is also extremely sensible in its operation upon arseniate of potash; and it seems most decidedly to distinguish this salt from the above solution, or arsenite of potash; for the colour of this precipitate is much darker and more inclined to red or brick colour." Med. and Phys. Journal, vol. xxii. p. 450.

In his letter to you, Sir, Mr. Hume says, "I shall now expect to be told that I have also been forestalled respecting my test for arsenic, that the arseniate of silver, the *brick red* coloured compound, had been prepared by others, &c.;" and that "M. Klaproth had frequently got hold of it, analyzed it, regenerated the same compound by means of nitrate of silver; but was so cruel and unlucky as to disregard the silver as *a test*, always preferring the *acetate of lead*, even to the end of the second volume of his valuable analytical researches." Mr. Hume is the best judge of the foundation on which this expectation is raised; and that it may not be disappointed, I refer him to the following passage from p. 151, vol. ii. of Klaproth's work; "To be more convinced that this precipitate was an arseniated lead, I drenched it with water, and digested it with half its weight of *sulphuric acid*. The liquor separated by filtration contained uncombined arsenic acid. I neutralized it with soda, and treated one part of it with a solution of nitrated silver. This produced a copious precipitate of arseniated silver, which possessed the brick red colour peculiar to it." By Mr. Hume's indiscreet challenge of inquiry, and triumphant defiance of the result of it, he is reduced to a dilemma; from which, if he extricate himself, he will show that his talent for supporting his claims is commensurate to his facility in making them. If, as must be inferred from the above quotation in his letter, he have read Klaproth's essays, how could he prevail upon himself to publish as his own the discovery that nitrate of silver is a test of arsenic acid; when that author expressly asserts that he employed nitrate of silver "to be more convinced" that a precipitate he had obtained was arseniated lead? If on the other hand he have not read them, how could he venture to assert that Klaproth preferred the use of acetate of lead to that of nitrate of silver?

To many persons invective is a "cheap defence," and it costs no one less than Mr. Hume; in the use of it he is voluble, and seems justly conscious that he may employ it without injury to his reputation. Consistently with this style, he insinuates that I am "an hireling," actuated by "flattery, ambition, malevolence, or jealousy;" that I am "peevish," and evince "much malice and little candour."

Having in my present communication rescued another discovery from Mr. Hume's extensive grasp, and restored it to the rightful owner, I expect from him a repetition of the invectives of which he has been so liberal, with such scanty additions as even his well-stored vocabulary can now afford; but having, as I conceive, answered Mr. Hume's arguments, I shall hold myself excused from entering upon a mode of discussion, in which success is inseparable from disgrace.

I remain yours, very respectfully,

29, Poultry, Feb. 22, 1815. Richard Phillips.

Article IX.

A Memoir on Iodine. By M. Gay-Lussac.

(*Continued from* p. 214.)

Combination of Iodine with Chlorine.

Dry iodine rapidly absorbs chlorine, producing a heat which rises to 212°. The compound has in some parts a fine orange-yellow colour; in others, an orange-red. The yellow parts contain more chlorine than the red; they are likewise more volatile. Though I passed a great deal of chlorine over the iodine, yet the greatest part was not saturated. We shall see immediately by what characters this point can be determined. To the red compound of iodine and chlorine I give the name of *subchloruret of iodine*, though it does not appear to me to have fixed proportions. To the yellow compound I give the name of *chloruret.*

Both of these compounds speedily deliquesce in the air. The solution of the subchloruret is a deeper orange-yellow the more iodine it contains. The solution of the chloruret is colourless when the excess of chlorine is driven off, and then the mutual saturation of the two constituents seems to be complete. Both solutions are very acid, and destroy the colour of the solution of indigo in sulphuric acid. When the solution of the chloruret is saturated with an alkali, it is changed completely into iodate and hydro-chlorate. When too long exposed to the light, it becomes coloured. It dissolves a great quantity of iodine, and then assumes the characters of subchloruret. Heat disengages chlorine from it, and the iodine being then in excess, the liquid assumes the characters of subchloruret. The solution of the subchloruret is volatilized without decomposition. Light does not produce any further alteration upon it. When saturated with an alkali, it gives iodate and hydro-chlorate; but if the alkali be cautiously added, we obtain a precipitate of iodine, which disappears on the addition of more alkali, and then hydriodate and iodate are formed. Thus the subchloruret is characterized by the precipitation of iodine when an alkali is added, whereas the chloruret gives no such precipitate.

We obtain but little chloruret in a solid state, as I have already remarked; but it may be obtained with facility, and in great quantity, on solution in water. For that purpose nothing more is requisite than to saturate with chlorine a somewhat diluted solution of subchloruret. It is then exposed for some time to the sun till it loses its colour, or it may be put into a large bottle in which the air is continually renewed. By this means we obtain a very acid colourless liquid, having only a slight smell of chlorine, which destroys the colour of solution of indigo in sulphuric acid, though slowly, and gives an abundant precipitate of iodine when ammonia is poured into it. We cannot employ heat to drive off the excess of chlorine unless it be very moderate; for I have just observed that it converts the liquid into a subchloruret. When we wish to saturate a solution of subchloruret with chlorine, the liquid ought to be dilute; because, when concentrated, the process does not succeed. The subchloruret presents itself frequently, and possesses stability, while the other (to make use of the expression) has only an ephemeral existence.

When we pour hydro-chlorate of potash or barytes into a solution of the chloruret or subchloruret, it gives up its base to a portion of iodic acid which we may conceive to be present; but the hydrochloric acid becoming predominant, prevents a complete decomposition.

We have seen that the solution of chloruret is changed into iodate and hydro-chlorate, when saturated with an alkali. From this fact, and from the characters of the solution, we may suppose it to be a mixture of iodic and hydro-chloric acid. On the other side, as it deprives indigo of its colour, it would seem that the chlorine and iodine in it still preserve their properties entire. We may conceive it likewise to be a peculiar acid, which is decomposed when we saturate it with a base. I adopt the first supposition, because I compose exactly the solution of the chloruret when I mix iodic and hydro-chloric acids together. But I consider their elements as very mobile, and capable of taking a new arrangement according to circumstances. On this supposition the water is decomposed when the chloruret is dissolved in it. Its oxygen combines with the iodine, and its hydrogen with the chlorine. The inverse distribution could not take place, for the iodic and hydro-chloric acids are much more stable than the chloric and hydriodic acids; and it is a general law that, every thing else being equal, the strong compounds are always formed in preference to the weak.

If we take a given quantity of iodine, and act upon by an alkali, it will be divided into two very unequal parts. The smallest portion forms iodate; the greatest, hydriodate. If we wished to convert it entirely into iodate, we must begin by making it a chloruret; and after having dissolved it in water, we saturate it with the alkali which we wish to convert into iodate. The iodates of barytes, lime, and strontian, being very little soluble in water, will be obtained pure after some washings. The others must be separated from the hydro-chlorates by repeated crystallizations or by alcohol.

Of the Hydriodates.

In general these salts may be prepared by combining hydriodic acid with the bases; but we may obtain those of potash, soda, barytes, strontian, and lime, directly by treating iodine with these bases, employing the methods above described to separate them from the iodates which are formed at the same time. The hydriodates of zinc, iron, and in general of all the metals that decompose water, are obtained by dissolving the iodurets of these metals in water. We may put together the water, the iodine, and the metal, and by the application of heat the hydriodate is quickly formed. I do not propose to treat of all the hydriodates in detail, but merely to give their generic characters and their principal properties.

Sulphurous, hydro-chloric, and hydro-sulphuric acids, produce no change on the hydriodates at the usual temperature of the atmosphere.

Chlorine, nitric acid, and concentrated sulphuric acid, instantly decompose them, and separate the iodine.

With solution of silver they give a white precipitate insoluble in ammonia; with the pernitrate of mercury, a greenish-yellow precipitate; with corrosive sublimate, a precipitate of a fine orange-red, very soluble in an excess of hydriodate; and with nitrate of lead, a precipitate of an orange-yellow colour.

They dissolve iodine, and acquire a deep reddish-brown colour.

Hydriodate of Potash.

When a solution of hydriodate of potash is made to crystallize, the oxygen combined with the metal, and the hydrogen with the iodine, unite together, and form water, and we obtain crystals of ioduret of potassium similar to those of chloruret of sodium. This salt easily melts, and sublimes at a red heat. When heated in contact of air, it undergoes no alteration. It is more deliquescent than the hydro-chlorate of soda. 100 parts of water, at the temperature of 64°, dissolve 143 of the salt. We may consider it as a hydriodate while it is in solution in water; but when melted, or even dried, it is obviously an ioduret of potassium. I find that when ioduret of potassium is dissolved in water, and afterwards dried, its weight is not increased.

Ioduret of potassium is composed of

Iodine	100
Potassium	31·342

and the hydriodate of

Hydriodic acid	100
Potash	37·426

Hydriodate of Soda.

I obtained it in pretty large flat rhomboidal prisms. These prisms uniting together form larger ones, terminated in echelon, and striated longways, like those of sulphate of soda. They contain a great deal of water of crystallization, and yet are very deliquescent. Heat drives off this water, melts the salt, and then renders it somewhat alkaline. It does not sublime so easily as hydriodate of potash. 100 parts of water, at the temperature of about 57°, dissolve 173 of the salt. When dried, it must be considered as an ioduret of sodium. I found that 100 parts of iodate of soda give, when decomposed by heat, 24·45 of oxygen. From the data given by that analysis, we may conclude the composition of the ioduret of sodium and hydriodate of soda to be as follows:—

Ioduret of sodium	Iodine	100
	Sodium	18·536
Hydriodate of soda	Acid	100
	Soda	24·728

The hydriodates of potash and soda converted into iodurets by desiccation, are the only ones not altered when heated to redness in contact with the air. The reason is, that iodine decomposes the oxides of potassium and sodium.*

Hydriodate of Barytes.

This salt crystallizes in very fine prisms, very similar in appearance to the hydro-chlorate of strontian. After about a month's exposure to the air, I found it partly decomposed. Water dissolved the hydriodate coloured by iodine, and there remained undissolved subcarbonate of barytes. Hence the hydriodic acid is gradually destroyed by exposure to the air. Its hydrogen has formed water, and its iodine has been dissipated in the atmosphere, or has remained dissolved in the undecomposed hydriodate. The hydriodate of barytes, though very soluble in water, is but faintly deliquescent. When evaporated in a close vessel, and heated to redness, it does not melt, nor is its state of neutralization altered. If air, or, still better, if oxygen, be made to play on its surface when thus heated, vapours of iodine show themselves in abundance, and the salt becomes alkaline. I did not continue the experiment till the iodine ceased to be disengaged; but I presume that the hydriodate would be changed into a subioduret, as we have seen before that this was the compound obtained when iodine in vapour was passed over barytes at a red heat. I have said that iodine does not disengage oxygen from barytes; yet I have no doubt that a red heat changes hydriodate of barytes into ioduret of barium. I have passed hydriodic gas cooled to — 4° over barytes obtained from the recent calcination of the subnitrate; the barytes instantly became incandescent, and water made its appearance in the vessel; yet this barytes gave no oxygen gas when dissolved in water; nor did it undergo any alteration when I passed over its surface a current of dry hydrogen gas. I ascertained, likewise, that sulphur disengaged nothing; but that hydro-sulphuric gas produced much water when it combined with it.* We cannot, therefore, doubt that at a red heat, and even at a lower temperature, hydriodate of barytes is converted into ioduret of barium.

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* As iodine disengages oxygen from the oxides of lead and bismuth, it is evident that the iodurets of these metals will not be decomposed by the air at a red heat.

Ioduret of barium	Iodine	100
	Barium	54·735
Hydriodate of barytes	Acid	100
	Barytes	60·622

The hydriodates of lime and strontian are very soluble, and the first is exceedingly deliquescent. I have neither determined the shape of their crystals, nor the quantity of water necessary to dissolve them. The hydriodate of strontian melts below a red heat, while the hydriodate of lime requires a higher temperature for its fusion. If they are heated in close vessels they become only slightly alkaline; but if air or oxygen have access to them while hot, thick vapours of iodine are immediately exhaled. If we consider these compounds as iodurets, the calcium and strontium are oxydized, and abandon a portion of the iodine. If we consider them as hydriodates, the hydrogen of the acid must combine with oxygen, and water be formed. I endeavoured to ascertain whether this was the case by passing dry oxygen gas over hydriodate of lime at a red heat; but the muriate of lime through which the gas afterwards passed, did not increase sensibly in weight. Hence every thing leads us to consider the hydriodates that have been melted or dried, as converted into metallic iodurets. The hydriodate of lime made with hydriodic acid, may be dried in the air without being decomposed. On the contrary, what has been made with iodine and lime becomes deep coloured as it is concentrated, though we evaporate at a very moderate heat. The reason is, that this last hydriodate holds in solution a certain quantity of iodate, and these two salts have the property of decomposing each other, when brought to a certain degree of concentration. The hydrogen of the hydriodic acid and the oxygen of the iodic acid form water, and the iodine which is thus disengaged is dissolved in the undecomposed portion of hydriodate, and gives it a reddish brown colour. The whole hydriodate is not destroyed, because there is only a small quantity of iodate present; and when calcined in close vessels, it is completely freed from colour.*

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* The action of the hydrosulphuric gas was accompanied by a strong heat. The compound which was partly fused, being treated with hydrochloric acid, hydrosulphuric gas was disengaged, and a little sulphur precipitated. From this it is probable that a sulphuret with excess of sulphur is formed, and hydrogen disengaged. But as I employed a sulphuret which yielded a gas not totally absorbed by the alkalies, I could not ascertain the fact. Yet the abundant production of water which accompanies the combination of hydrosulphuric gas with barytes, and even with strontian, cannot be explained, except by admitting that these alkalies are reduced by the hydrogen in consequence of the united affinities of the oxygen for hydrogen, and of the metals for sulphur. But if this be the case, it is very probable that many metallic precipitates, which have been taken for hydrosulphates (hydrosulphurets) are only sulphurets. At a red heat, all the oxides which combine with sulphur give out water, and are changed into sulphurets when hydrosulphuric gas is brought in contact with them. This fact proves nothing against the existence of hydrosulphates at a low temperature. But hitherto there is not a single decisive experiment in proof of their existence; while the insolubility of them all seems to be a fact of an opposite nature. To confirm these conjectures, I dissolved a determinate weight of zinc in hydrochloric acid. I supersaturated the solution by ammonia, and precipitated by hydrosulphuric acid. The precipitate dried in the temperature of between 140° and 176° assumed the appearance of horn. Its weight was too great for a sulphuret, and too small for a hydrosulphuret. When heated to 212° it gave out water, and a new quantity was disengaged at a higher temperature. This experiment is not entirely decisive; but from the appearance of the precipitate I think it was a hydrate. At all events, this experiment is rather favourable than otherwise to my conjecture.

Hydriodate of ammonia results from the combination of equal volumes of ammoniacal gas and hydriodic gas. It is usually prepared by saturating the liquid acid with ammonia. It is nearly as volatile as the hydrochlorate of ammonia; but it is more soluble and more deliquescent. I have obtained it crystallized in cubes. When heated in close vessels only a very small portion of it is decomposed. What sublimes is greyish black. If it be sublimed in contact of air, a much greater proportion of it is decomposed, and it becomes more or less coloured. It may be deprived of its colour by adding a little ammonia, or by exposing it to dry air. In the last case, the iodine to which it owes its colour is gradually volatilized.

The hydriodate of magnesia, formed by uniting its constituents together, is deliquescent, and crystallizes with difficulty. When heated to redness in close vessels, the magnesia abandons the acid in the same way as it abandons the hydrochloric acid. Having heated together iodine, magnesia, and water, to ascertain whether hydriodate and iodate was formed, as happens with the other alkalies, I obtained a flocky compound, which exactly resembled well prepared kermes. The liquid which coovered it was scarcely coloured, and I ascertained in it the presence of hydriodate and iodate of magnesia, but in very small quantity. When evaporated, a flea-coloured matter is deposited on the sides of the vessel, quite similar to that of which I have spoken; and towards the end of the process, the liquid acquires a deep colour. This phenomenon is owing (as in the case of lime) to the mutual decomposition of the hydriodate and iodate of magnesia when they reach a certain point of concentration, but it is much more marked with magnesia.

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* To evaporate or calcine the hydriodates without the contact of air, I put them into a retort, to the beak of which I fit a tube, which, after having received the form proper for collecting gases, rises at its extremity parallel to the descending branch, and assumes nearly the shape of the letter U. When the aqueous vapour has expelled all the air of the retort, I place the ascending branch under a glass jar filled with hydrogenous azotic gas, above the level of the water.

The flea-coloured matter is decomposed when put upon burning coals. Iodine is disengaged, and magnesia remains. Potash decomposes it likewise. When boiled in water, its colour is not changed, but the liquid is found to contain a little iodate and hydriodate. If the quantity of water be considerable, pure magnesia remains, and the water contains iodate and hydriodate.

From these facts it appears that the flea-coloured matter is an ioduret of magnesia, and that its existence in water depends upon the property which the iodate and hydriodate of magnesia have of mutually decomposing each other when concentrated to a certain point. When the water is in great quantity, no ioduret of magnesia appears; but it is deposited as the concentration advances.

This phenomenon does not take place with the iodates and hydriodates of potash and soda. It begins to show itself with those of strontian. It becomes more sensible with the iodate and hydriodate of lime, and is very conspicuous with those of magnesia. But this last alkaline basis has a weaker affinity than the others; and it is perhaps because the oxides of zinc and iron, &c. have a still weaker, that they do not condense a sufficient quantity of hydriodic and iodic acids to prevent them from acting on each other, and that in treating them with iodine no iodates and hydriodates are formed, though these salts may be obtained separately.

(To be continued.)

ARTICLE X.

ANALYSES OF BOOKS.

An Attempt to establish a pure scientific System of Mineralogy by the Application of the Electro-Chemical Theory, and the Chemical Proportions. By J. Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm. Translated from the Swedish Original by John Black. 1814.

The doctrine of chemical proportions, though but recently introduced into chemistry, has produced a great reform in the science, and has given birth to a degree of accuracy, both in experimenting and reasoning, which has already placed chemistry on a footing with the mathematical sciences. Nobody has contributed more to produce this reform than Professor Berzelius. He has made the most numerous and accurate analyses which we at present possess, and has pointed out several general conclusions, which serve greatly to facilitate this kind of investigation. His activity, which surpasses that of any other chemical experimenter of the present day, has led him to apply the doctrine of chemical proportions to all the different departments of the science. The object of the present little work is to show that minerals are all real chemical compounds, that every species consists of constituents combined according to the laws of chemical proportions, and that they are susceptible of an accurate chemical arrangement into classes, orders, genera, and species, according to the nature of the substances of which they are composed.

The work bears evident marks of the great abilities and extensive knowledge of the author, and must suggest many important and useful ideas to every mineralogist who will read it with sufficient attention; though perhaps a more deliberate consideration of the subject, and a more minute acquaintance with the details of mineralogy, might have led to a modification of some of the opinions which Professor Berzelius has advanced; for instance, he seems to rate the knowledge of the external characters of minerals very low, and to consider the whole of the science of mineralogy as confined to an acquaintance with the constituents of which every mineral is composed. But it is necessary to recollect that, before the chemical analysis of any mineral can be of importance to the science, or lead to any useful inferences, we must be sure that the specimen which we subject to analysis belongs really to the species which we suppose, and that it is quite pure and unmixed with any other mineral. Now this knowledge can only be acquired by an acquaintance with the external characters of minerals—a branch of knowledge which must therefore precede all useful chemical analysis. Hence it must always serve as the basis of our mineralogical knowledge. In fact, the labours of the chemist, who applies his practical skill to minerals, can only be of utility when he takes care to make his experiments upon correct and pure specimens. If a chemist, for example, analyze a specimen of *mica*, and publish the result under the name of *an analysis of talc*; or if he give the name of *stilbite* to what in reality is *mesotype*, his labours, instead of being useful, must be injurious to mineralogy. Yet these mistakes have been committed by chemists of acknowledged skill. The same injurious effects arise from the analysis of impure specimens, as when a mixture of *felspar* and *quartz*, or of *felspar* and *garnet*, is analyzed under the name of *felspar*. Unless I am much mistaken, errors of this kind have been lately committed by some of the most accurate analysts of the present day.

The knowledge of the constituents of minerals is always interesting, and in many cases indispensable. Thus the art of mining is founded on the knowledge of the different metals which may be extracted from the different ores. But to conceive that the whole science of mineralogy consists in a knowledge of the constituents of minerals, and that every thing else is of no consequence, is what no person can possibly do who has taken the requisite pains to make himself acquainted with the science. The *diamond* was as accurately distinguished by its external properties, and was applied to as many uses by our predecessors, who were ignorant that this mineral consists entirely of pure *carbon*, as it is by us, who are acquainted with that fact—a fact which must be admitted to be curious and important; but not to constitute every thing of any value with respect to the mineralogy of the diamond. *Gypsum* was well known by its properties, and was applied to all the purposes for which it is used at present, before Margraaf and Lavoisier ascertained it to be a compound of sulphuric acid and lime. A mineralogist may be very well acquainted with the characters of gypsum, capable of distinguishing it from all other minerals, and aware of the different uses to which it is applied, though he be ignorant of the constituents which enter into its composition.

We must not, therefore, confine the science of mineralogy to the mere knowledge of the constituents of minerals. It includes many other particulars of great importance, and has frequently got the start of chemical analysis in its conclusions. Thus *calcareous spar* and *arragonite* were considered by mineralogists as two distinct species, even when the most expert chemists were unable to discover any difference in their composition. When chemical analysis shall have arrived at a state of perfection, we may expect to find it agree in every respect with the conclusions drawn from the external characters; but in its present imperfect state, such discrepancies cannot be avoided; and when they do occur, it is but reasonable to give the superiority to the deductions from the external characters, as less likely to mislead us than an imperfect chemical analysis.

The object of Berzelius in the present little work is to show that all mineral species are really chemical compounds, composed of ingredients combined in definite proportions, and capable of being classified into orders, genera, and species, according to their composition, just as may be done with the salts. Though numerous analyses of minerals exist, yet it must be confessed that these definite proportions, this chemical composition according to the atomic theory, can be perceived only in a small number of individuals; while the great body of the mineral kingdom seems to bid defiance to the application of the laws of chemistry. But this discordance Berzelius considers, and I believe with justice, as only apparent, and not real. He ascribes it to three causes: 1. The inaccuracy of experimenters. 2. The mechanical mixture of foreign bodies with the chemical compounds, in consequence of the situation in which they were when they became solid or crystallized. 3. The deposition of two different compounds in contact with each other, which gives occasion to a form different from that of either of the compounds. Thus *arragonite* owes its form to the deposition of an atom of carbonate of strontian in contact with carbonate of lime at the time of its crystallization.

The minerals which it has hitherto been impossible to bring under the laws of chemical combination are the stones, composed chiefly of silica, alumina, lime, and oxide of iron, united in various proportions. Professor Berzelius conceives that in these minerals the silica performs the function of an acid, and that it is chemically combined with the other earths or oxides which perform the function of bases. According to this notion, which has likewise been advanced by Mr. Smithson, the stony bodies are in reality salts. But they are often of a more complicated nature than the salts composed of the common acids and bases; for in the *siliciates*, as Berzelius terms these stones, we find not only the combination of silica with one base, but with two, three, or four bases, and often in various proportions, so as to constitute subsiliciates or supersiliciates.

If we suppose silica, alumina, magnesia, and lime, to be composed of one atom of oxygen and one atom of base, as I have done in the table published in the second volume of the *Annals of Philosophy*; or of two atoms of oxygen and one atom of base, as Berzelius has done, it is obvious that the number of integrant particles in any stony body may be determined by ascertaining the proportion which the oxygen of the various constituents bear to each other. Thus suppose we examine a mineral composed of silica and lime, and find that the oxygen in the silica is three times as great as in the lime, it follows that the mineral is composed of three integrant particles of silica and one integrant particle of lime; so that it may be termed a trisiliciate. This is the mode which Berzelius has taken to determine the constitution of the various stony bodies. It has the advantage of being at once easy and accurate, supposing us acquainted with the composition of the different earths.

Berzelius supposes silica to be composed of 50·36 silicon + 49·64 oxygen. If therefore we consider it as a protoxide, it follows that the weight of an atom of silicon is 1·007, and the weight of an integrant particle of silica 2·007. He considers alumina as composed of 53·3 aluminum + 46·7 oxygen. The following table exhibits the weight of an atom of the different bases according to the analyses employed by Berzelius in this work.

	Weight.		Weight.
Silicon	1·007	Silica	2·007
Aluminum	1·141	Alumina	2·141
Magnesium	1·631	Magnesia	2·631
Calcium	2·571	Lime	3·571
Barium	8·523	Barytes	9·523

This table will enable the reader to calculate the composition of the different stony bodies, which have been subjected to an accurate analysis. For example, *schaalstein*, or *table spar*, is a *hydrous bisiliciate of lime*; *somnite*, or *nepheline*, is a *siliciate of alumina*; one of the species of *calamine* analyzed by Smithson, and composed of silica and oxide of zinc, is a *siliciate of zinc*; *cerite* is a *siliciate of cerium*. Berzelius gives examples of more complex siliciates. Thus *ichthyophthalmite* is a hydrate consisting of *water* combined with five integrant particles of *trisiliciate of lime* and one integrant particle of *trisiliciate of potash*: and *malacolite* consists of one integrant particle of *bisiliciate of lime* and one integrant particle of *bisiliciate of magnesia.*

Berzelius proposes to divide minerals into as many *families* as there are simple substances known, which amount to about 46. The families are divided into orders, according to the different electro-negative bodies with which the most electro-positive are combined: as, for example, 1. Sulphurets: 2. Carburets: 3. Oxides, &c. He gives the following examples of this arrangement, which will make it more intelligible.—Silver Family. 1*st order:* Pure silver. 2*d order:* Sulphurets. 3*d order:* Stibiets, consisting of antimonious silver ore and *silberspies-glanz.* 4*th order:* Tellurets, containing the various ores of tellurium. 5*th order:* Aurets, containing electrum and auriferous silver. 6*th order:* Hydrargyrets, containing native amalgam. 7*th order:* Carbonates. 8*th order:* Muriates.—Iron Family. 1*st order:* Native iron. 2*d order:* Sulphurets. 3*d order:* Carburets. 4*th order:* Arseniets. 5*th order:* Tellurets. 6*th order:* Oxides. 7*th order:* Sulphates. 8*th order:* Phosphates. 9*th order:* Carbonates. 10*th order:* Arseniates. 11*th order:* Chromates. 12*th order:* Tungstates. 13*th order:* Siliciates. 14*th order:* Tantalates. 15*th order:* Titaniates. 16*th order:* Hydrates.—Family of Aluminium. 1*st order:* Sulphates. 2*d order:* Fluates. 3*d order:* Fluo-siliciates. 4*th order:* Siliciates. 1*st subdivision:* Single siliciates, comprehending nepheline, collyrite, &c. 2*d subdivision:* Double siliciates, comprehending beryl, emerald, Euclase; mealy zeolite, lomonite; harmotome, or cross-stone; mesotype, schorl; felspar, lepidolite, leucite. 3*d subdivision:* Triple and complex siliciates, comprehending prehnite, koupholite, mica.

Five appendices are added to the dissertation itself. In the first, Berzelius gives the ground of his calculations, as they have been already explained in his paper, On the Cause of Chemical Proportions, printed in the second and third volumes of the *Annals of Philosophy.* The second appendix exhibits the number of atoms of oxygen existing in the various oxides. In the third appendix he explains the different symbols which he employs in his discussions. The fourth contains a valuable set of experiments on magnetic iron ore, proving it to be a compound of black and red oxides of iron. This valuable paper shall be inserted in a subsequent number of the *Annals.* We shall likewise in a future number insert the fifth appendix, in which Berzelius determines the proportion of oxygen contained in glucina.

ARTICLE XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Feb. 23.—At this meeting a part of a paper by Sir H. Davy, entitled, Some Experiments and Observations on the Colours used in Painting by the Ancients, was read.

March 2.—At this meeting the conclusion of Sir H. Davy's paper was read. The following are the results of his observations:—

The experiments were made on colours found in the baths of Titus, in the baths of Livia, and other ruins of Ancient Rome, as well as of Pompeii. Some of them were discovered in vases found below the ruins of the Palace of Titus. These were identified with the colours employed in various fresco paintings in this palace.

Three kinds of red were found in a vase two years ago; the first, approaching to orange, was minium; the second, a dull red; and the third, a purplish red, were ochres. Another red, found in fresco paintings, was plainly vermilion, called minium by the Romans, who gave the name of cerussa usta to our minium, which Sir H. Davy supposes to have been a natural carbonate of lead burnt. The yellows are ochres diluted with different quantities of chalk, and yellow oxide of lead, or massicot. No sulphurets of arsenic were detected, though they were known to Pliny and Vitruvius.

Lumps of a deep blue frit were collected among the baths of Titus, which were composed of soda, silica, and oxide of copper. All the blues were composed of this compound, and the intensity of their colour was reduced with carbonate of lime. Sir Humphry supposes this to be the blue colour described by Theophrastus as having been discovered by an Egyptian King, and as manufactured at Alexandria. The cœrulium of Vitruvius was made by heating sand, natron, and copper.

No traces of indigo have been detected, though it was known to the ancients. Cobalt was observed colouring glass.

The greens are carbonate of copper, except one which approaches to olive. This is the common green earth of Verona. Sir H. thinks it not improbable that many of the carbonates of copper were originally acetites.

In the bottom of a broken vase found in the baths of Titus a substance externally cream-coloured, internally carmine, existed, which was combustible, and left about $\frac{1}{30}$ of siliceous, aluminous, and calcareous earths. It is regarded as a lake, though it is impossible to determine whether it be animal or vegetable.

The browns are mere oxides of iron, and sometimes mixtures of iron and manganese.

No cerusse was found among the whites, which were generally carbonates of lime and fine white clays.

The ground on which the painting was executed was powdered marble, cemented with lime, which was polished. No wax or animal gluten was discovered on any of the paintings.

March 9 and 16.—At these two meetings a paper was read from Dr. Brewster, on the laws which regulate the polarization of light by reflection from the second surfaces of transparent bodies.

LINNÆAN SOCIETY.

At the meeting of the 7th March a paper was read from Mr. Sowerby, on a branched fossil found in Green Sand, Warminster, to which he proposes to give the name of pedunculum lobatum. A figure was also exhibited.

At the same meeting a paper was read from the Rev. Patrick Keith on the epidermis of plants, in which, after stating the general analogy pointed out by Grew, and other early vegetable physiologists, between the epidermis of plants and animals, he examines the doctrine especially supported by M. Mirbel, who regards that substance as merely the external surface of the parenchyma acted upon and hardened by the atmosphere. He then shows that this last opinion is unfounded, and finally endeavours to establish the general analogy adopted by the early writers.

A memoir from Dr. Leach on the distribution of the orders composing the class Insecta, with descriptions of some new genera and species was read. In the following table an outline of his classification is inserted.

CLASS INSECTA.

Subclass I.—AMETABOLIA.

Insects undergoing no metamorphoses.

Order I. *Thysanura.*—Tail armed with setæ.
Order II. *Anoplura.*—Tail without setæ.

Subclass II.—METABOLIA.

Insects undergoing metamorphoses.

Century I. *Elythroptera.*—Insects with elytra.
Odontostoma.—Mouth with mandibles.

* Metamorphoses incomplete.

Order III. *Coleoptera.*—Wings transversely folded; elytra crustaceous, covering the wings, with the suture strait.

** Metamorphosis nearly coarctate.

Order IV. *Strepsiptera.*—Wings longitudinally folded; elytra coriaceous, not covering the wings.

*** Metamorphoses semi-complete.

Order V. *Dermaptera.*—Wings longitudinally and transversely folded; elytra somewhat crustaceous, abbreviated, with the suture strait.

Order VI. *Orthoptera.*—Wings longitudinally folded; the internal margin of one elytron covering the same part of the other; elytra coriaceous.

Order VII. *Dictuoptera.*—Wings longitudinally folded twice or more; elytra coriaceous, nervous, one decussating the other obliquely.

Siphonostoma.—Mouth with an articulated rostrum.

Order VIII. *Hemiptera.*—Elytra somewhat crustaceous, or coriaceous; towards the apex generally membranaceous, horizontal, one decussating the other obliquely. Metamorphoses half complete.

Order IX. *Omoptera.*—Elytra entirely coriaceous, or membranaceous, and meeting obliquely, with a strait suture. Metamorphoses semi-complete, or incomplete.

Century II. *Medamoptera.*—Insects without wings or elytra.

Order X. *Aptera.*—Mouth with a tubular sucking rostrum. Metamorphoses incomplete.

Century III. *Gymnoptera.*—Insects with wings, but no elytra.

Cohors I. *Glossostoma.*—Mouth with a spiral tongue.

Order XI. *Lepidoptera.*—Wings four, membranaceous, with pterigostea, covered with meal-like scales.

Cohors II. *Gnathostoma.*—Mouth with maxillæ and lip.

Order XII. *Trichoptera.*—Wings four, membranaceous, with pterigostea, and hairy.

Cohors III. *Odontostoma.*—Mouth with mandibules, maxillæ, and lip.

Order XIII. *Neuroptera.*—Four highly reticulated wings, generally equal in size; anus of the female without a sting, or compound borer.

Order XIV. *Hymenoptera.*—Four venose wings, hinder ones smallest; anus of the female with a sting, or with a compound borer or oviduct.

Cohors IV. *Siphonostoma.*—Mouth tubular, formed for sucking.

Order XV. *Diptera.*—Wings, and halteres or balancers two.

At the meeting of the 21st of March, a paper containing some additions to the Novi Systematis Ornithologici Prolusio, by M. Vieillot, concluded at a former meeting.

GEOLOGICAL SOCIETY.

Jan. 20, 1815.—The reading of Dr. Macculloch's paper on Glen Tilt was concluded.

The remarkable appearances attending the junction of the granite and of the schistose rocks at Glen Tilt were first pointed out by Dr. Hutton, and were considered by him as affording strong proofs of the original fluidity of granite, and of its having forcibly intruded itself into the situation which it now occupies at a period subsequent to the formation of the schist with which it is intermixed.

The supporters of the Wernerian theory who have visited this remarkable valley have denied the accuracy of some of Dr. Hutton's descriptions, and have called in question the justness of the conclusions deduced by him from these appearances. The advocates for Dr. Hutton's theory have brought forward counter statements, Hence it becomes a matter of considerable importance that the real nature of the facts appealed to by both parties should be clearly ascertained.

Most of the previous observers appear to have confined their researches to particular parts in the bed of the Garry (the river which flows through the Glen), where the rocks may be observed with least trouble, and where the junctions of the granite with the schist are the most obvious. In the present memoir the entire bed of the river from one end of the Glen to the other, where it was accessible, is particularly described, as well as the slopes of the mountain which form the sides of the Glen.

The right side of Glen Tilt consists for the most part of red granite, passing sometimes by the intermixture of crystals hornblende into sienite. This granite may be traced to Ben Deary, and thence to the great body of granite forming the centre of the Highlands, and of which body the right bank of Glen Tilt may be considered as the southern boundary.

The surface of the granite is irregularly undulated and protuberant. Incumbent on the granite are several stratified rocks, the lowest of which appears to be quartz rock, which is covered by, and alternates with, mica-slate, either pure or passing into gneiss, with clay-slate and with granular lime-stone, either the common blue variety, or white marble, which latter sometimes contains veins and concretions of noble serpentine, and is sometimes interstratified with thin beds of tremolite and of sahlite.

The beds lying in the granite are sometimes broken off abruptly; and this latter rock making its appearance in the intervals frequently gives the appearance of an alternation of granite with the stratified rocks that in reality lie only on its surface.

The stratified rocks are for the most part very regular in their position; but where they are in contact, or approach very near to the granite, very remarkable disturbances, and other changes, are observed to take place. The lime-stone is much indurated, and more siliceous than usual. Sometimes it contains small grains of reddish-brown felspar; sometimes the lamina of the lime-stone are separated, and the interstices are filled up with granite; sometimes veins of various magnitude pass from the body of the granite into the superincumbent beds, occasioning flexures, fractures, and various intimate and fantastical intermixtures of all the adjacent rocks; sometimes, on the other hand, lumps and masses of the stratified rocks are to be found involved in the granite.

Quartz rock is not always the immediate covering of the granite; on the contrary, the several varieties of schist, and even lime-stone, are sometimes found in that position, a circumstance that appears to be irreconcileable with the theory of the stratified rocks having been deposited on a basis of pre-existing granite, but easily accounted for on the theory of the forcible irruption of the granite from below in a fluid state: in which case it would rupture and penetrate the superincumbent beds, more or less, according to the force originally impressed upon it, and to the degree of resistance offered by the varying thickness and solidity of the stratified rocks.

A letter from the Rev. Richard Hennah, jun., dated Jan. 19, 1815, was read.

The object of this letter is to state that, in addition to the information contained in the former letters of this gentleman, (read Nov. 18, 1814,) concerning the occurrence of organic remains in the Plymouth lime-stone, he has recently found many well-defined shells of the genus turbo, and of other genera, at the east end of the Hoe under the citadel; and also fragments of shells in a quarry at Cat-down.

A memoir on the native oxide of uranium of Cornwall, by W. Phillips, Esq. M. G. S. was read.

Uranite in green tabular transparent crystals, imbedded in ferruginous quartz, has been found by Mr. Phillips, at Carharrock, near St. Die. It has also been found by him in very minute crystals, varying in form and colour, and accompanied by pitch blende, at Tincroft mine near Redruth; also in well defined crystals, and accompanied by pitch blende, at Tol Carne mine.

At Huel Jewel this mineral occurs on red copper ore; and in a single instance has been found on the surface of Wavellite from Stenna gwyn near St. Austle. Specimens of extraordinary beauty have been discovered within the last three or four years at Gannis lake copper mine near Callington, on decomposed granite.

The larger crystals of this mineral are all deeply striated; and even the most minute ores, although seemingly well defined, are not capable of being accurately measured by the reflecting goniometer. From some very satisfactory clearages which Mr. Phillips has obtained, it appears to have natural joints parallel to the lateral faces, and to the two diagonals of a tetrahedral prism.

The paper concludes with a description, illustrated by drawings, of 46 varieties of crystalline form.

A paper, entitled Supplementary Observations on Quartz Rocks, made in 1814, by Dr. Macculloch, was read.

This rock forms the larger portion of the great valley of the Tunnel, where it alternates with micaceous and with argillaceous schist, with lime-stone, and with porphyry. It may be observed passing insensibly into granite, and, when it alternates with mica-slate, the proportion of quartz rock will be found to prevail in the vicinity of granite; and that of mica-slate at a distance from the granite. In Ben Gloe it occurs incurvated and contorted; and on Cairn Gower are some beds consisting wholly of large and small rounded pebbles. In Mar forest it alternates with large incomplete garnets. In Glen Tilt it presents a finely laminated structure, like the schistose sand-stones of the coal strata, and is divided by natural joints into rhomboidal tables. In Ben Gloe it occurs of a beautiful pink colour; and in Glen Fernat it is mixed with scales of mica forming an avanturine capable of receiving a good polish.

March 3.—The reading of Mr. Horner's paper on the Geology of the South Western part of Somersetshire was begun.

Article XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Middlesex Hospital.—Dr. Merriman's next Course of Lectures on Midwifery and the Diseases of Women and Children will be delivered at the above hospital during the months of April and May. The Introductory Lecture will be read on Monday, April 10, at half-past ten o'clock.

II. *Smithson Tennant, Esq.*

It is with much concern that we announce the death of Smithson Tennant, Esq. F. R. S. and Professor of Chemistry in the University of Cambridge; a man in whom genius, talents, and virtue, were united in their highest forms. Although his industry was checked by a frame naturally weak, and a languid state of health, his acquirements in science were remarkably general, and in many branches profound.

The circumstances of Mr. Tennant's death were most afflicting. He was returning from France, where he had been several months, and was waiting at Boulogne for a favourable wind. He had actually embarked on Wednesday the 22d of February, but the vessel was obliged to put back; and it was determined, if the weather should be tolerable, to make another trial in the evening. During the interval Mr. T. proposed to a German officer of distinction, whom he had accidentally joined on the road, and who was also going to England, to ride with him to Buonaparte's Pillar, near Boulogne. In returning he deviated a little to look at a fortification near the road. But as they were attempting to pass a draw-bridge, which, owing to some neglect, was not properly secured, the bridge gave way, and they were precipitated into the trench. The officer fortunately escaped without any serious hurt; but Mr. Tennant was found fallen under his horse, and was taken up speechless, his skull and one of his arms being considerably fractured. He was conveyed with difficulty to the hospital at Boulogne, as the nearest and most convenient place to receive him, and expired in half an hour. His remains were interred at Boulogne.

A fuller account of this interesting philosopher is preparing, and will be given in an early number of the *Annals*.

III. *Dr. Seetzen, the German Traveller.*

Dr. Seetzen is little known in this country, except through a translation, by the Palestine Society, of a brief account by him of the countries adjoining the lake Tiberias, the Jordan, and the Dead Sea. As his merits are very extraordinary, it will be interesting to give a short sketch respecting him.

Ulrich Jasper Seetzen, born in the Russian lordship of Jever, is the son of an affluent farmer. He was educated at Gottingen, where he took a degree in medicine; and having steadily regarded as the final end of his studies the exploring the interior of Africa, he prepared himself by the study of natural history in all its branches, Eastern languages, and astronomy, geography, &c. In 1802 he left Gotha, accompanied by a young man of the name of Jacobsen, and went to Vienna, whence he proceeded with some difficulty to Constantinople, where he arrived in December, 1802. Thence he went to Smyrna, which he quitted in October, 1803, having previously explored the adjacent country. He then resided at Aleppo, and afterwards at Damascus; afterwards he examined the geological structure of the country towards Jerusalem, Lebanon, and Latitibanon. In 1805 he travelled through Arabia into Egypt, and finally arrived at Cairo. During residence at this last place (for two years) he transmitted his very valuable observations in separate dissertations to Germany, where many have been published, and many remain in the possession of his relations. In 1809 he quitted Cairo, returned into Arabia, and in Nov. 1810 he was at Mokka, since which no correct account of him has reached this country. A report has been received of his death, but in a very suspicious form. It would be gratifying to his friends to know his fate, as they would then be enabled to give the whole of his valuable researches to the world, a part of which only has hitherto appeared in the journal of his liberal friend Baron Von Zach.

IV. *Gas Light.—Conveyance of Steam in Pipes.—Smallest Angle at which Objects are perceptible.*

(To Dr. Thomson.)

DEAR SIR,

I have been much gratified by the information which has come out respecting the saccharometer, in consequence of my inquiry in your *Annals*. The "Philosophical Intelligence" is a most interesting part of that work.

Permit me to suggest to you that accidents, arising from philosophical negligence, if I may use the expression,—such as the late *explosion* of a brewer's store cask, and a recent accident with a gas-light apparatus at Birmingham,—deserve to be recorded in your *Annals*, for obvious reasons. A friend of mine was going to light two large manufactories with coal gas, but since the late accident he has been undetermined whether to proceed or not. Will it be presuming too much to ask your opinion respecting the new mode of illumination with gas? The opinion of so experienced a chemist and philosopher would have a most beneficial influence. And what sort of coal is to be preferred for producing the gas in question? I presume the diameters of the conduit pipes have hitherto been taken at random, and that the pressure on the reservoir of gas for sending a stream through the pipes has not yet been accurately observed, or at least not published.

I am also desirous to inquire how far steam has been known to be conveyed, or how far it probably can be conveyed, for heating water; and how large the conducting pipes should be to produce a required effect. Is any damage to be expected from the sudden condensation of the steam on its first application to the cold water: I have been told that the pipes are liable to burst from this cause; a circumstance which I think Count Rumford never mentions in his essays on this subject.

One more query, and I have done. Has any experiment been published which ascertains the smallest object or angle that is perceptible to a sound unassisted eye, or an eye assisted with a given magnifying power? In a philosophical work translated from the French I have read that the eye is incapable of perceiving an object which subtends an angle of less than half a minute of a degree.

I remain, Sir, most respectfully, your obliged servant,

Feb. 3, 1815. M.

V. *Meteorological Table. From the Register kept at Kinfauns Castle, N. Britain. Supposed Lat.* 56° 23¼′. *Above the Sea* 129 *feet.*

1814.	Morning, 8 o'clock. *Mean height of*		Evening, 10 o'clock. *Mean height of*		Depth of Rain	No. of days	
	Barom.	Ther.	Barom.	Ther.	In. 100	Rain or Snow.	Fair.
Jan.	29·78	24·64	29·80	25·39	·60	11	20
Feb.	30·06	34·03	30·04	34·50	·63	8	20
March	29·90	36·48	29·91	36·80	·85	17	14
April	29·90	46·43	29·90	46·10	1·75	18	12
May	30·15	47·06	30·16	44·77	·75	8	23
June	30·16	53·06	30·02	50·50	·57	6	24
July	29·93	59·35	29·94	57·10	1·72	15	16
Aug.	29·91	56·77	29·93	54·86	2·60	18	13
Sept.	30·10	51·90	30·10	52·66	·57	8	22
Oct.	29·82	44·54	29·84	44·51	1·02	11	20
Nov.	29·74	38·70	29·74	38·16	2·30	12	18
Dec.	29·65	34·80	29·66	35·38	2·23	18	13
Aver. of year.	29·925	43·980	29·920	43·394	15·59	150	215

ANNUAL RESULTS.

MORNING.

Barometer.			Thermometer.		
Observations.	*Wind.*			*Wind.*	
Highest, May 11	S E	30·64	July 24	S E	64
Lowest, Jan. 16	S E	28·70	Jan. 14	W	12

EVENING.

Highest, May 10	E	30·65	July 25	S E	65
Lowest, Mar. 2	S E	28·91	Jan. 15	W	12

Weather.	*Days.*
Fair	215
Rain or Snow	150
	365

Wind.	*Times.*
N and N E	3
E and S E	109
S and S W	65
W and N W	188
	365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 14th January	7°
Hottest, 24th July	76
Mean for 1814	45·168

Result of three Rain Gages.	*In.* 100.
No. 1. On a conical detached hill above the level of the sea 600 feet....	33·84
— 2. Centre of garden, 20 feet	20·05
— 3. Kinfauns Castle, 129 feet	15·59
Mean of the three gages	23·61

VI. *Royal Medical Society of Edinburgh.*

The Royal Medical Society of Edinburgh having agreed to appoint a Committee for the purpose of receiving the communications of members, and of others through their medium, who may favour the Society with interesting facts and experiments in Medicine, or with unusual appearances in morbid dissection, beg leave earnestly to invite the members to transmit such communications to the Society as soon as possible.

The Committee, consisting of six extraordinary members resident in Edinburgh, together with the four Presidents ex officio, will proceed immediately to consider such papers as may be transmitted to them, with an ultimate view to publication.

VII. *On Imaginary Cube Roots.*

(To Dr. Thomson.)

SIR,

You sometimes insert algebraical communications in your *Annals of Philosophy*. I have lately discovered that all numbers have four imaginary cube roots, which is contradictory to the generally accepted theory, that "every equation has as many roots as it has dimensions, and no more."

The imaginary cube roots of the number 64 are $-2 \pm \surd -12$; but the following quantity raised to the third power shows that it is also a cube root of the same number 64.

$$\begin{array}{l}
-\frac{3+\surd-3}{2}+\surd-(\frac{13}{2}-\frac{3}{2}\surd-3) \\
-\frac{3+\surd-3}{2}+\surd-(\frac{13}{2}-\frac{3}{2}\surd-3) \\
\hline
\frac{6+6\surd-3}{4}-\overline{3+\surd-3}\,\surd-(\frac{13}{2}-\frac{3}{2}\surd-3) \\
-\frac{13}{2}+\frac{3}{2}\surd-3 \\
\hline
-5+3\surd-3-\overline{3+\surd-3}\,\surd-(\frac{13}{2}-\frac{3}{2}\surd-3) \\
\quad -\frac{3+\surd-3}{2}\qquad+\qquad\qquad\surd-(\frac{13}{2}-\frac{3}{2}\surd-3) \\
\hline
12-2\surd-3+\overline{3+3\surd-3}\,\surd-(\frac{13}{2}-\frac{3}{2}\surd-3) \\
24+2\surd-3-\overline{5-3\surd-3}\,\surd-(\frac{13}{2}-\frac{3}{2}\surd-3) \\
\hline
36+0+(-2+6\surd-3)\surd-(\frac{13}{2}-\frac{3}{2}\surd-3)
\end{array}$$

To find the value of the last term, the first member must be squared, the second member must be multiplied into the square, and the sign of the square root prefixed to the result. Thus,

$$\begin{array}{r}
-2+6\sqrt{-3}\\
-2+6\sqrt{-3}\\
\hline
4-24\sqrt{-3}\\
-108\qquad\qquad\quad\\
\hline
-104-24\sqrt{-3}\\
-\frac{13}{2}+\frac{3}{2}\sqrt{-3}\\
\hline
676+156\sqrt{-3}\\
108-156\sqrt{-3}\\
\hline
784\qquad\qquad\quad
\end{array}$$

And prefixing the sign it becomes √ 784 = 28, which added to 36 found before gives 64.

I am, Sir, your obedient servant,

Tubney Park, March 5, 1815. JAMES LOCKHART.

VIII. *On the Use of the Cerebellum and Spinal Marrow.*

(To Dr. Thomson.)

SIR, *London, Feb.* 14, 1815.

In the 26th Number of your Journal is announced a discovery of the use of the cerebellum and structure of the spinal marrow, by Dr. John Cross of Glasgow. The following are the words of this announcement:

"I was led about a year ago to conclude, that as the cerebrum is the fountain of sensation and intellect, the cerebellum must be the organ which supplies with nervous energy the face and other parts of the head, extrinsical of the brain, &c." Again, "I found that the spinal marrow is uniformly composed of four longitudinal divisions; two larger, anterior, which may be traced into the cerebrum, and two smaller, posterior, which may be traced into the cerebellum. I suppose that the two anterior portions are the organ of feeling; the two posterior, of motion."

Now, Sir, this, though more vaguely stated, is so closely imitative of a statement which I actually published nearly *six years ago*, (in Archives of Universal Science, vol. iii. for July, 1809,) that I am compelled to beg you will do me the justice to point it out. The following is that statement.

"From the peculiar opposition which subsists between the situation of the face and cerebellum, we are entitled to expect a similar opposition in their functions. As the face, therefore, occupied by the chief organs of sense, is the seat of sensation, so we might expect *the cerebellum* to be *the organ of volition.* This supposition receives additional force from the consideration that, as the organs of sense and the cerebellum are the first and the last portions of the nervous system; so sensation and volition are the first and the last of its functions. But this supposition is completely confirmed, when we recollect that the degrees of voluntary power always bear a close analogy to the various magnitudes of the cerebellum. In fishes, for instance, which possess amazing locomotive power, the cerebellum is often larger than the hemispheres! (P. 176, Op. Cit.)

"The spinal marrow actually consists of *four columns*, of which *two are anterior and two posterior*. In the anterior columns terminate the anterior fasciculi of all the spinal nerves; and *these columns* themselves, passing through the medulla oblongata, crura cerebri, and corpora striata, *terminate in the hemispheres* of the cerebrum. The anterior fasciculi of the spinal nerves are, therefore, their ascending fasciculi, and the anterior columns of the spinal marrow are its ascending columns. *From the cerebellum again descend the posterior columns* of the spinal marrow, and from these proceed the posterior fasciculi of the spinal nerves. The posterior columns of the spinal marrow are, therefore, its descending columns, and the posterior fasciculi of the spinal nerves are their descending fasciculi. *Hence it is that these nerves and almost all the nerves of the body, have filaments of ascending and filaments of descending impression; are at once nerves of sensation and nerves of locomotion.*" (Page 142.)

These quotations are, I believe Sir, sufficiently decisive as to the original author of those discoveries, which Dr. Cross intends to make the subject of his promised work.

On this subject I may take this opportunity of observing, that my friend Dr. Spurzheim, (in his recent and very valuable work on the Anatomy, Physiology, and Physiognomy of the Brain,) has most certainly erred in denominating the cerebellum the organ of amativeness; for *physical love no further depends on the cerebellum than that its degree is of course inseparably connected with the degree of voluntary power which is the proper function of that organ*; and hence it is, that the bull and stallion, having larger cerebella and more energetic voluntary power, have also a stronger propensity to physical love than the ox and cow, or the gelding and mare. Notwithstanding this objection however, I reckon that work the most important accession to our anatomical knowledge which ever has been made by any philosopher.*

I remain, Sir, with great respect, your obedient Servant,

ALEXANDER WALKER.

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* Since writing the above, Sir, I observe in the 27th number of your Journal a letter addressed to you by Dr. Leach, in which he indicates the large work of Gall and Spurzheim on the brain, published at Paris in 1807, as the original source of the discoveries so unfairly claimed by Dr. Cross. In answer to Dr. Leach, I need only say, 1. That Gall and Spurzheim's work actually contains no such statements; and 2. That such statements are even in direct opposition to their doctrines.

IX. *On the Organic Remains in Plymouth Lime-stone.*

(To Dr. Thomson.)

SIR, *Citadel, Plymouth, March* 9, 1815.

In the last Number of the *Annals of Philosophy* there is a letter on my having found madrepores and shells in the lime-stone rocks at Plymouth. I should probably not have troubled you with a reply, had it not been stated that no shells had been observed in those rocks "in situ." It appears Dr. Leach is not aware of the discovery of a bed of lime-stone, abounding in shells, in the dock-yard; and probably he is equally unacquainted with my communications to the Geological Society, and the specimens which I have sent for the use and inspection of its members. In addition to these, I could show him many varieties of shells which I have obtained from various parts of the rocks in this neighbourhood. I might add, about the same time.

With respect to the madrepores, Dr. Lockyer, with some other friends, accompanied me in one of my visits to the quarry where I first found them; and he was of opinion, I remember, that they had more the appearance of imbedded stalactites than of animal remains; and, as I was of a different opinion, in sending some specimens to the Geological Society, I thought they might be interesting at least, if they did not tend to any useful purpose.

As to the specimen in my possession of what Dr. Leach calls madreporite of a fine blood-red colour, I never considered it as such, but of a quite different nature. It was given to me several years ago, as coming from the neighbourhood of Stonehouse, or Devil's Point, and believing it to be the case, I have made many attempts to discover its native bed, though without success.

I have no time for uninteresting or useless writing; but deeming the field of Science and Natural History open to all who are disposed to enter it, I shall continue my researches as hitherto, without giving, I hope, at least without intending to give, offence to any one; and certainly without assuming to myself the merit (if any) which others may think due exclusively to their own labours.

I have the honour to remain, Sir, your obedient Servant,

RICHARD HENNAH, JUN.

ARTICLE XIII.

Scientific Books in hand, or in the Press.

Dr. Ronalds, of Coventry, is preparing for the Press a Translation of the celebrated little work of Cabanis on Certainty in Medicine.

Mr. Huish will shortly publish a Scientific and Practical Treatise on the Management of Bees, with Plates.

Mr. Parkes, the author of the Chemical Catechism, has in the Press a series of Chemical Essays, to be published in four pocket volumes, including a variety of Explanatory Notes, and an Index. It will be illustrated with more than 20 Copper-Plate Engravings.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1815.	Wind.	Barometer.			Thermometer.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
2d Mo.										
Feb. 1		29·45	29·36	29·405	48	39	43·5			☾
2	E	29·62	29·45	29·535	46	36	41·0			
3	W	29·62	29·39	29·505	49	39	44·0		·13	
4	S W	29·80	29·39	29·595	54	36	45·0			
5	S E	29·92	29·74	29·830	50	38	44·0			
6	S W	29·74	29·55	29·645	47	42	44·5		4	
7	Var.	29·70	29·55	29·625	52	36	44·0		—	
8		29·62	29·49	29·555	50	38	44·0	·15	·15	
9	S E	29·66	29·63	29·645	47	37	42·0			○
10		29·63	29·43	29·530	46	40	43·0		4	
11	Var.	29·40	29·28	29·340	51	39	45·0		·11	
12	S W	29·51	29·37	29·440	50	48	49·0		·21	
13	S W	29·60	29·40	29·500	52	32	42·0			
14	E	29·65	29·40	29·525	50	37	43·5		—	
15	Var.	29·65	29·56	29·605	51	42	46·5		—	
16	S W	29·56	29·50	29·530	54	35	44·5	·18	·16	
17	N W	30·26	29·50	29·880	52	33	42·5		2	☽
18	S W	30·26	30·16	30·210	51	34	42·5			
19	S W	30·16	29·71	29·935	48	45	46·5		—	
20	W	29·82	29·55	29·685	54	42	48·0		·26	
21	N W	30·15	29·82	29·985	56	48	52·0			
22		30·20	30·15	30·175	57	39	48·0			
23		30·15	30·00	30·075	51	40	45·5	·27		●
24	W	30·00	29·97	29·985	52	47	49·5			
25	S W	29·97	29·92	29·945	53	46	49·5			
26	N W	30·40	29·92	30·160	54	29	41·5		5	
27		30·47	30·44	30·455	48	25	36·5			
28	S E	30·44	30·15	30·295	45	31	38·5			
3d Mo.										
Mar. 1	S E	30·19	30·15	30·170	52	33	42·5	·20	1	
		30·47	29·28	29·785	57	25	44·43	·80	1·18	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Second Month.—1, 2. Misty dull weather. 3. Cloudy: wind and rain in the night. 4. Temp. 49° at the time of observation: fine: *Cumulus,* beneath hazy *Cirrus.* 5. *Cirrostratus* and haze at sun-rise, and much dew, which did not evaporate: a very fine day: temp. 56° in the sun: p. m. *Cumulostratus.* 6. The same, followed by rain. 7. a. m. Overcast, dripping: p. m. wind N.W.: groups of *Cumulostratus, Cumulus,* and *Cirrostratus,* occupying the whole S. from one to two, coloured light indigo, with red haze above, and attracting the smoke. 8. p. m. The wind rose at S. with the usual hollow sound: a beating rain from that quarter ensued. 9. Misty, from a diffused *Cirrostratus,* and cloudy above. Water from a well, which might be 10° warmer than the air, emitted a visible steam. 10. Cloudy: dripping at intervals. 11. The same: dark *Nimbi* passed in the S., the wind being W.: p. m. 12. a. m. Misty: cloudy: wind and showers. 13. A wet windy morning, succeeded by a very fine day: *Cumulus,* with *Cirrus.* 14. A very moist *Cirrostratus,* a. m.: then fine, with various clouds: a little misty rain at night. 15. Much dew: the rain kept off till evening. 16. Windy: wet: a lunar corona. 17. Morning clear, with dew: then *Cumulus,* which becoming dense inosculated with the clouds above and with the smoke: a little rain p. m.: lunar corona. 18. Slight hoar frost: haze, passing to *Cirrus* and *Cirrostratus.* 19. Heavy *Cumulostrati* through the day: much wind. 20. Windy: wet: lunar halo at night. 21, 22. *Cumulostratus:* windy. 23. Fine. 24. Ramified *Cirrostratus,* indicating wind, which followed. 25. Blustering wind, with *Cumulostratus,* and strong evaporation: a little rain notwithstanding. 26. Showers, which laid the dust: a sudden clearing in the W. at sun-set. 27. Very white frost: a fine day, out of the smoke. 28. Hoar frost, and a frozen mist: a serene day.

Third Month.—1. Hoar frost: *Cirrus* in a fine elevated veil, passing to *Cirrostratus,* and exhibiting between ten and eleven *a solar halo:* a very fine day, the pollution of the smoke excepted.

RESULTS.

Winds variable, but for the most part Southerly.

Barometer:	Greatest height	30·47 inches
	Least	29·28
	Mean of the period	29·785
Thermometer:	Greatest height	57°
	Least	25
	Mean of the period	44·43

Evaporation, 0·80 inch. Rain, 1·18 inch.

Tottenham,
Third Month, 10, 1815.

L. HOWARD.

ANNALS

OF

PHILOSOPHY.

MAY, 1815.

ARTICLE I.

Biographical Account of Joseph Black, M. D. F. R. S. E. &c. Professor of Chemistry in the University of Edinburgh.

By Thomas Thomson, M. D. F. R. S.

THE materials from which the following account was drawn were first given to the public in the preface to Dr. Black's lectures, edited by Professor Robison. Mr. Robison informs us that he was indebted for most of his facts "to a paper read to the Royal Society of Edinburgh by the near relation of Dr. Black, Dr. Adam Ferguson, Professor of Mathematics in the University, and well known in the republic of letters by works of the very first rank."

Dr. Joseph Black was born in France on the banks of the Garonne in the year 1728. His father, Mr. John Black, was a native of Belfast, in Ireland, but of a Scotch family, which had been some time settled there. Mr. Black resided for the most part at Bourdeaux, where he carried on the wine trade. He married a daughter of Mr. Robert Gordon, of Hilhead, in Aberdeenshire, who was also engaged in the same trade at Bourdeaux.

The mother of Dr. Black, and the mother of Mr. James Russel, Professor of Natural Philosophy in the University of Edinburgh, were sisters; and the mother of Dr. Adam Ferguson was their aunt, a circumstance which was the origin, though not the cement, of a friendship subsisting between them through life.

In the year 1740 young Black, then in the 12th year of his age, was sent to Belfast, that he might have the education of a British subject. After finishing his grammar school education, he went, in 1746, to the University of Glasgow. Dr. Cullen had commenced his great literary career, and having made choice of philosophical chemistry as a field still untrodden, was delivering lectures upon that science in the University of Glasgow. These lectures caught the congenial fancy of young Black, who speedily became a zealous chemist, and the favourite pupil and friend of his master. Mr. Black had made choice of medicine as the profession to which he proposed to attach himself; and in 1750 or 1751 he went to the University of Edinburgh to finish his medical studies. Here he lived in the house of his cousin German, Mr. James Russel, Professor of Natural Philosophy, in whose society he must have passed his time both agreeably and profitably.

At this period the opinions of the medical professors were divided about the manner in which certain lithontriptic medicines acted in alleviating the excruciating pains of the stone. One of these medicines was lime-water. They all belonged to the class of bodies called *caustic*, and their efficacy was ascribed to this *causticity*. Now this causticity was always induced, either directly or indirectly, by the fire. Thus lime-stone in its natural state possesses no caustic properties; but by exposure to a strong heat it is converted into the caustic substance called quick-lime. The investigation of the nature and cause of this causticity was considered as very important. It drew the particular attention of Mr. Black. He investigated the subject with his accustomed precision and coolness; and having ascertained it in a satisfactory manner, made it the subject of his inaugural dissertation in 1754, when the degree of Doctor of Medicine was conferred upon him by the University of Edinburgh. Next year he published his experiments upon *magnesia alba, quick-lime, and other alkaline substances*, in which the whole subject was developed at length.

Just at this time Dr. Cullen was removed to Edinburgh, and the chemical chair in Glasgow became vacant. Dr. Black's experiments on magnesia and quick-lime, which afforded by far the finest specimen of chemical investigation hitherto offered to the public, secured him that chair. Accordingly he was appointed Professor of Anatomy, and Lecturer on Chemistry, in the University of Glasgow. Not considering himself as sufficiently qualified for the anatomical class, he exchanged tasks with the Professor of Medicine. While in Glasgow, therefore, his lectures on the institutes of medicine constituted his chief task. He engaged likewise in the practice of medicine; and from the sweetness of his manners, and the goodness of his heart, soon became a favourite practitioner.

While in Glasgow, he brought to maturity his speculations concerning heat, which had occupied his attention from the very commencement of his medical studies. They constitute one of the most important additions ever made to science, and furnish the most delightful proofs of the wisdom and beneficence of the Author of nature.

Dr. Black continued in the University of Glasgow from 1756 to 1766. In that year Dr. Cullen, Chemical Professor in Edinburgh, was appointed Professor of Medicine, and thus a vacancy was made in the chemical chair of that University. There was but one wish with respect to a successor. Indeed, when the vacancy happened in 1756, by the death of Dr. Plummer, Dr. Black's reputation stood so high, that, had it depended on the University, he would have been appointed to the chair. He had now greatly added to his claim of merit by his important discovery of the procedure of Nature in producing fluidity and vapour; and he had acquired the high esteem of every one by the singular moderation and scrupulous caution which marked all his researches. Dr. Black was appointed to the chemical chair of Edinburgh, to the general satisfaction of the public; but the University of Glasgow sustained an irreparable loss. In this new scene his talents were more conspicuous, and more extensively useful. The number of his pupils underwent a progressive and annual increase during the whole time that he was Professor. Many of these pupils were from the workshop of the artist or manufacturer, and had not enjoyed the advantage of a liberal education. Yet such persons, in the opinion of Dr. Black, constituted by no means the least important part of his class. He laboured, therefore, with the greatest assiduity, to bring his lectures to a level with this least informed part of his audience; and thus every year he rendered them more and more elementary. His lectures were always listened to by his audience with inexpressible delight. His voice was low; but sweet and distinct. His language was simplicity itself; but always apposite, and never vulgar. His experimental illustrations were exactly suited to the object in view; and carried full conviction to the mind of the spectator: there was no glare, no parade, no showman exhibition; but an attic elegance and simplicity highly delightful to a refined and cultivated mind. I describe the lectures such as I listened to them myself, about the year 1796. But at that period Dr. Black's vigour was nearly gone. Indeed his state of health was such that he was obliged to employ an assistant to help him in his experiments, and to lecture for him occasionally. But even with all these disadvantages, the effects of his lectures were such as I can never forget: they made an impression on my mind which no time can efface.

Dr. Black's health had been always delicate. The least exertion brought on a cough, with a spitting of blood. This obliged him to remain a tranquil spectator of the chemical discoveries which were constantly pouring in from all quarters, and to leave it to others to explore the tempting fields which he had originally laid open. Towards the latter period of his life, when undue advantages were taken by certain foreigners of the discoveries which he had made, without any acknowledgment of obligations to the original discoverer, he was urged by his friends to lay an historical detail of the whole of his labours before the public. He began this task more than once; but was always obliged to desist almost immediately, in consequence of the illness brought on by this unusual exertion of thought.

By abstaining from all exertion, by living in the most abstemious manner, and by constant, though moderate exercise, he contrived to enjoy an almost uninterrupted, though feeble state of health, and to prolong his life to a considerable old age, happy to the last day, and capable of enjoying the conversation of a few select friends. His only apprehension was that of a long continued sick bed; and this perhaps less from any selfish feeling than from the humane consideration of the trouble and distress occasioned to attending friends; and never was this modest and generous wish more completely gratified. "On the 26th Nov. 1799, and in the 71st year of his age, he expired without any convulsion, shock, or stupor, to announce or retard the approach of death. Being at table with his usual fare—some bread, a few prunes, and a measured quantity of milk diluted with water; and having the cup in his hand when the last stroke of his pulse was to be given, he set it down on his knees, which were joined together, and kept it steady with his hand in the manner of a person perfectly at ease; and in this attitude expired, without spilling a drop, and without a writhe on his countenance; as if an experiment had been required to show to his friends the facility with which he departed. His servant opened the door to tell him that some one had left his name, but getting no answer, stepped about half way towards him, and seeing him sitting in that easy posture, supporting his bason of milk with one hand, he thought that he had dropped asleep, which he had sometimes seen happen after his meals. He went back and shut the door; but before he got down stairs some anxiety, which he could not account for, made him return and look again at his master. Even then he was satisfied, after coming pretty near him, and turned to go away; but again returning, and coming quite close to him, he found him without life.

"So ended a life which had passed in the most correct application of reason and good sense to all the objects of pursuit which Providence had prescribed to his lot, with many topics of agreeable recollection, and few things to ruffle his thoughts. He had long enjoyed the tender and affectionate regard of parents whom he loved, honoured, and revered; with the delightful consciousness of being a dutiful son, and being cherished as such—one of a family remarkable for sweetness of disposition and manners, he had lived with his brothers and sisters in terms of mutual love and attachment. He had never lost a friend, but by the stroke of mortality, and he felt himself worthy of that constancy of regard. He had followed a profession altogether to his taste; and had followed it in a manner, and with a success, which procured him the esteem and respect of all competent judges, and set his name among the most eminent; and he was conscious that his reputation was not unmerited; and with a success in point of emolument which secured the respect even of the ignorant; which gave him the command of every rational gratification, and enabled him to add greatly to the comforts of the numerous descendants of his worthy parents—heirs, not only of their name, but likewise of their unambitious moderation and amiable simplicity of character."

The publications of Dr. Black (omitting his inaugural dissertation) amount only to three short papers; for respecting his discoveries and speculations concerning heat, he published no account at all. In the year 1803, or three years after his death, his lectures were published by Dr. John Robison, Professor of Natural Philosophy in the University of Edinburgh, who undertook the task of editor at the request of Dr. Black's friends. They appear to me to be a pretty fair representation of his lectures such as they were about the year 1796; and though it was by no means doing justice to Dr. Black to give to the world lectures never intended for publication, and just at a period too when the science had assumed a new form, quite different from that under which he had been accustomed to view it; yet they convey a very happy view of the elegant simplicity of manner by which he was distinguished, of the very apt illustrations by which the different subjects that he treated were elucidated, and of the true philosophical caution with which all his conclusions were drawn.

His Experiments upon Magnesia Alba, Quick-Lime, and other Alkaline Substances, were published in 1755, and constitute in my opinion one of the very best specimens of analytical investigation ever offered to the public. It was known that when lime-stone is exposed to a violent heat for a sufficient time, it is converted into quick-lime, a substance which has a strong caustic taste, gives a green colour to vegetable blues, corrodes animal and vegetable substances, &c. If pearl-ash be mixed with three or four times its weight of quick-lime, and agitated for some hours in a quantity of water, the quick-lime is converted into chalk, and the pearl-ash becomes exceedingly caustic and corrosive. These facts were sufficiently known; but no satisfactory explanation of them had been offered, though it was generally believed that the fire had imparted something to the lime to which it was indebted for its causticity. Dr. Black demonstrated, by the most decisive experiments, that lime-stone is a compound of two substances; namely, quick-lime and a peculiar species of air which acts the part of an acid, and to which he gave the name of *fixed air*. The fire disengages this air, and drives it off. The quick-lime remains in a state of purity, and exhibits its natural caustic properties. Pearl-ash is in like manner a compound of pure potash and fixed air. Quick-lime has a stronger affinity for fixed air than potash has. Accordingly it separates the fixed air from the alkali, and is converted into lime-stone; while the alkali, thus freed from the acid with which it was united, exhibits its natural caustic properties in all their energy. Such is the outline of the theory which Dr. Black establishes in his dissertation. He establishes likewise the peculiar properties of magnesia, and shows that it differs from every other earthy body.

Dr. Black's speculations respecting heat have had such an effect upon the progress of the science, that it would be unpardonable not to notice them here. A very good account of them will be found in the first volume of his lectures. Indeed, his lectures on heat constituted the most finished, and by far the most valuable, part of his course. It is well known that the freezing point of water is 32°, that whenever the thermometer sinks below 32° water begins to freeze, and whenever it rises above 32° ice and snow begin to melt. At the first view of the subject one would be disposed to expect that as soon as the thermometer sinks below 32° the whole water would immediately become ice, and that when it rises above 32° the ice would be as speedily converted into water; but every body knows that these speedy changes never take place. In cold weather a crust of ice is formed upon the surface of rivers and lakes; and if the cold continue, this crust becomes gradually thicker. But unless the water be very shallow, it is very seldom or never totally converted into ice. The warm weather returns while a considerable portion of the water of the lake is still unfrozen. We remark the same slowness in the conversion of ice into water. When snow is accumulated in great quantities in mountainous countries, it resists the united action of the sun and the wind for weeks, or even months. It is always melting, indeed, but it melts very slowly; and in some cases the cold weather returns again before the liquefaction is completed. Such were the facts which had been obvious to all the world from the beginning. Dr. Black was the first person who examined them closely and endeavoured to explain them. According to him, water is a compound of two substances—ice and heat. It cannot freeze or be converted into ice till it has parted with its heat; and as the heat makes its escape but slowly, the water freezes but slowly. Ice, on the other hand, can only be converted into water by combining with a certain quantity of heat; and as this combination takes place but slowly, the ice melts but slowly. This view of the subject Dr. Black confirmed by simple but satisfactory experiments. The heat which thus renders water fluid he called *latent heat*, because its presence is not indicated by the thermometer. He showed that the latent heat of water is 140°. He ascertained likewise that fluidity in all cases is owing to the combination of latent heat with the body becoming fluid.

It is well known that water and other liquids, when exposed to heat, increase in temperature till they become boiling hot, but after that their temperature remains stationary. They gradually indeed boil away, and are converted into steam or vapour, an elastic fluid possessing many of the properties of air; with this difference, that when exposed to the action of cold it is again converted into the very liquid from which it was originally produced. Dr. Black showed that vapour or steam is a combination of the liquid from which it was produced and latent heat. The latent heat of the vapour of water or steam he found about 950°. It was this doctrine respecting the nature of steam that led Mr. Watt to his great improvements on the steam-engine—improvements which have been of incalculable benefit to the manufactures of Great Britain.

Dr. Black was the first person who pointed out that every substance is possessed of a peculiar *specific heat*, or that different bodies have different *capacities* for heat. This subject was afterwards further investigated by Dr. Irvine, of Glasgow, and by Mr. Wilcke, of Stockholm.

A very short paper by Dr. Black was published in the 65th volume of the Philosophical Transactions, for the year 1775, giving an account of some experiments showing that recently boiled water begins to freeze more speedily than water that has not been boiled. He found that if the unboiled water be continually stirred, it begins to freeze as soon as the boiled water. He gives the following explanation of the phenomenon. Water by boiling is deprived of a portion of air. When exposed to the atmosphere it begins to absorb this air, and continues to do so till it has recovered its original quantity. This absorption produces a disturbance in the water, not indeed sufficient to be perceived by the eye, but sufficient to prevent it from becoming colder than 32°, without beginning to freeze.

The only other paper written by Dr. Black was published in the second volume of the Transactions of the Royal Society of Edinburgh. It is an analysis of the Geyzer and Rikum springs in Iceland. A quantity of the water of these springs was brought from Iceland by Sir John Thomas Stanley, and sent to Dr. Black. This paper may be taken as a model of the proper manner of examining mineral waters. The following were the constituents found in 10,000 grains of each of these waters:—

	Rikum.	Geyzer.
Soda	0·51 gr.	0·95 gr.
Alumina	0·05	0·48
Silica	3·73	5·40
Common salt	2·90	2·46
Sulphate of soda	1·28	1·46
	8·47	10·75

ARTICLE II.

On the Doctrine of Fluxions. By Alexander Christison, Esq. Professor of Humanity in the University of Edinburgh.

(To Dr. Thomson.)

MY DEAR SIR,

It is very much to be regretted that many students at the universities of North Britain acquire no knowledge of fluxions. They seem to think it extremely difficult to obtain even the first principles of that important branch of mathematical science. In what follows, (which, if you think proper, you may insert in the *Annals of Philosophy*,) I intend to solve inductively the fluxional problem as extensively as Newton has demonstrated it in the second lemma of the second book; then to demonstrate that problem rigorously in the manner of the ancients, independently of infinitesimals, of motion, or of vanishing quantities; and, lastly, to subjoin some observations.

I am, my dear Sir, yours faithfully,

Edinburgh, March 20, 1818. ALEX. CHRISTISON.

Of Fluxions.

In consequence of repeated trials, I have long thought that a boy, duly prepared, passes from common algebra into fluxions as easily as he does from multiplication into division.

In solving the fluxional problem three things are to be distinguished—the conception, the notation, the demonstration.

1. With regard to the conception, I shall, in order to be easily understood, proceed as if I were questioning a learner. Ask him thus—If a straight line, A D, fig. 1, move parallel to itself at right angles along D C, blackening the parallelogram A F, whose side A E or B F is 5; and reddening the parallelogram E C, whose side E D or F C is unit; at what rate does it always blacken the one parallelogram and redden the other? He will answer—As 5 to 1. The conception is much aided, at first, by his imagining that the two parallelograms are generated of different colours.

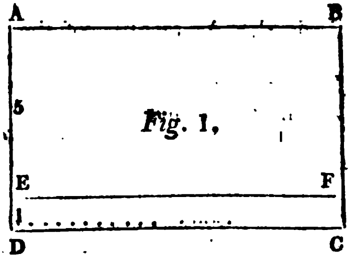


Fig. 1,

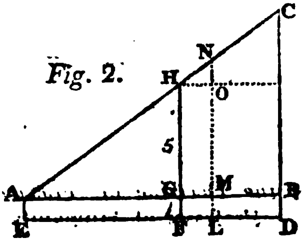


Fig. 2.

Ask him now thus—If a straight line move parallel to itself, at right angles along E D, fig. 2, so that it can generate only the parallelogram A D, whose side A E is unit, and the triangle A B C half a square, while all the rest of the space is covered; at what rate does it, at H F = 6, generate the triangle and the parallelogram? He will answer—As 5 to 1; or as 5 × 1 : 1 × 1; that is, as the area of the parallelogram H M is to the area of the parallelogram G L; but he will probably add, that the instant before the rate was less, and that the instant after it will be greater. He may be told, that it is not the rate the instant before, nor the instant after, which is wanted. He will now understand that, if the line F H were to proceed for ever of the same length, it would generate two parallelograms which would have always to one another the rate of 5 to 1. If, therefore, x represent the base or the perpendicular of the triangle, the rate will be as x to 1.

2. With regard to the notation, if x and 1 be both multiplied by any quantity whatever, their rate will not be changed; instead of x to 1, therefore, we may employ $x\,\dot{x}$ to $1\,\dot{x}$, $\dot{x}$ being any line more than nothing, and less than infinite: $x\,\dot{x}$, then, is the fluxion of the triangle, and $1\,\dot{x}$ the corresponding fluxion of the parallelogram; and as the triangle is the half of a square, the fluxion of a square whose side is x is to the fluxion of a parallelogram whose side is 1 as $2\,x\,\dot{x} : 1\,\dot{x}$.

If now the following series of fluents be set down, the learner will easily continue the fluxions.

Fluents.		Fluxions.
1	....	0, for 1 being invariable has no fluxion.
x	....	$1\,\dot{x}$
$x\,x$	....	$2\,x\,\dot{x}$
$x\,x\,x$	....	$3\,x\,x\,\dot{x}$
x^4	....	$4\,x^{4-1}\,\dot{x}$
......		
x^n	....	$n\,x^{n-1}\,\dot{x}$

A learner who sees in the series of the fluxions above the two laws of the three first terms, that of the numeral coefficients, and that of the letters, will be able to continue the series to any length, and to give the general expression $n\,x^{n-1}\,\dot{x}$ as the fluxion of x^n; because he observes that the numeral coefficients increase by unity, and that there are as many letters in the fluxion as in the corresponding fluent, with the last letter always dotted.

If the result be expressed in words, we have the following rule for finding the fluxion of any power of a variable quantity.

Multiply the fluxion of the root by the exponent of the power, and the product by that power of the same root whose exponent is less by unity than the given exponent.

By this rule the fluxion of $x^{\frac{m}{n}}$ is $\frac{m}{n}\,x^{\frac{m}{n}-1}\,\dot{x}$; of $x^{-\frac{m}{n}}$ is $-\frac{m}{n}\,x^{-\frac{m}{n}-1}\,\dot{x}$; of $x^m\,y^n$ is $m\,y^n\,x^{m-1}\,\dot{x} + n\,x^m\,y^{n-1}\,\dot{y}$, when both x and y vary, by considering first x as variable, and then y as also variable; of $\frac{x^m}{y^n} = x^m\,y^{-n}$ is $\frac{m\,y^n\,x^{m-1}\,\dot{x} - n\,x^m\,y^{n-1}\,\dot{y}}{y^{2n}}$.

From all that has been said, the fluxional or differential calculus may, in the case of one variable quantity, be defined a method for finding the rate of change in a quantity, and its dependance or function. Thus the rate of change in x and its function x^n, is as $\dot{x} : n\,x^{n-1}\,\dot{x}$, or as $1 : n\,x^{n-1}$.

As this is not a treatise, but a short essay, I say nothing of second fluxions, which bear the same relation to first fluxions that first fluxions bear to their fluents; nor of exponential and logarithmic quantities; nor of the arithmetic of sines; but I refer for information to some of the authors afterwards mentioned.

3. With regard to the demonstration, I think that the view already exhibited leaves no doubt in the mind of the learner; but a rigorous demonstration should be given, in order to enable him to reply to every objection.

Newton's second lemma of the second book seems to afford a demonstration that, while it is brief and comprehensive, is convincing, if the reduction to absurdity by Robins, vol. ii.; or by Hales, in the Logarithmic Writers, vol. v. p. 133 and 134; be subjoined to Newton's case first; if the process from particular to general be admitted in his case third; and if the momentum be admitted instead of the fluxion.

In a department of science so important and extensive as fluxions, the demonstrations of various authors should, I think, be studied by the learner; such as those of Simpson, Maclaurin (though extremely tedious), Euler, l'Huilier, Bossut, Vince, Dealtry, Lacroix, and Lagrange.

Any function of a variable quantity may be represented by the ordinate of a curve of which x is the abscissa. Let $y = x^n$ be a function of x, and let x become $x + i$; then $y = x^n$ will become $y^1 = \overline{x + i}|^n = x^n + n\,x^{n-1}\,i + \frac{n\,.\,n - 1\,x^{n-2}}{1\,.\,2}\,i^2 + \text{\&c.}$ If we subtract the first equation from the second, and divide both sides by i, we shall have $\frac{y^1 - y}{i} = n\,x^{n-1} + \frac{n\,.\,n - 1\,x^{n-2}}{1\cdot 2}\,i + \text{\&c.}$ Now it is evident that i, and consequently $y^1 - y$, which depends on i, may be so diminished that $n\,x^{n-1}$ may differ less from $n\,x^{n-1} + \frac{n\,.\,n - 1\,x^{n-2}}{1\,.\,2}\,i + \text{\&c.}$ than by any assigned quantity how small soever; and when in $\frac{y^1 - y}{i}$, i and $y^1 - y$ vanish, $\frac{O}{O}$ seems equal to $n\,x^{n-1}$. But this conclusion, says Lagrange, " presents no idea."

Prop. $\frac{FG}{EF}$ is equal to $n\,x^{n-1}$, fig. 3. Construct the figure in which T G is the tangent. Let $AI = x$; $IK = i$; $EI = y$; $KH = y^1$; bisect E F in O, and through O draw P N parallel to E I or K H; and through P draw E P M; let $PO = {}^1y$; $EO = {}^1i$; $FM = y^1$: then by similar triangles $PO : OE :: MF : FE$, or ${}^1y : {}^1i :: y^1 : i$; therefore $\frac{{}^1y}{{}^1i} = \frac{y^1}{i}$; and if E O be bisected, we shall

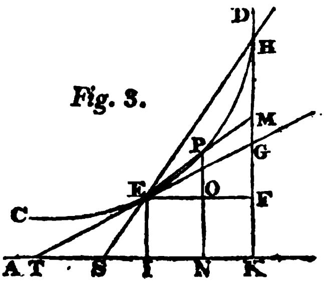


Fig. 3.

have $\frac{{}^{11}y}{{}^{11}i} = \frac{y^{11}}{i}$. Now it is evident that F G is the limit of y^{1}, y^{r1}, &c.; for y^{1}, y^{11}, &c. may approach, by continued bisections, nearer the point G than by any assigned difference, how small soever, but can never pass that point. $\frac{\mathrm{GF}}{\mathrm{EF}}$ is the geometrical limit of $\frac{y^{1}}{\mathrm{i}}$, $\frac{y^{11}}{\mathrm{i}}$, &c. which are successive values of $\frac{y^{1}-y}{i}$, while i is diminished by continual bisections; as $n\,x^{n-1}$ is the algebraical limit.

That $n\,x^{n-1}$ and $\frac{\mathrm{FG}}{\mathrm{EF}}$, the limits of $\frac{y^{1}-y}{i}$, are equal to one another, may be proved thus:—If they be not equal, let $\frac{\mathrm{FG}}{\mathrm{EF}}$ be the greater, and let D be the difference between $\frac{\mathrm{FG}}{\mathrm{EF}}$ and $n\,x^{n-1}$; then, because $\frac{y^{1}-y}{i}$, or its equal $\frac{y^{1}}{\mathrm{i}}$, &c. is always greater than $\frac{\mathrm{FG}}{\mathrm{EF}}$, $\frac{y^{1}-y}{i}$ cannot approach nearer to $n\,x^{n-1}$ than by more than D, but $\frac{y^{1}-y}{i}$ approaches nearer to $n\,x^{n-1}$ than by any assigned difference; therefore $\frac{\mathrm{FG}}{\mathrm{EF}}$ is not greater than $n\,x^{n-1}$; and in the same manner it may be proved not to be less; wherefore $\frac{\mathrm{FG}}{\mathrm{EF}}$ is equal to $n\,x^{n-1}$. Q. E. D.

If $\dot{x}$ be put for E F, and $\dot{y}$ for F G, then $\frac{\dot{y}}{\dot{x}} = n\,x^{n-1}$.

4. *Observations.*—This reasoning seems to me to remove Lagrange's objection lately mentioned, and to do so by employing an incremental fraction $\frac{{}^{1}y}{{}^{1}i}$, &c. equal to an approximating fraction $\frac{y^{1}}{\mathrm{i}}$, &c. of which approximating fraction the denominator is always $\mathrm{i} = \dot{x} = \mathrm{E\,F}$, the numerator continually approaches to $\mathrm{F\,G} = \dot{y}$, and the vanishing quantity is the difference between the numerator y^{1}, &c. and $\mathrm{F\,G} = \dot{y}$.

This approximating fraction $\frac{y^{1}}{\mathrm{i}}$, &c. is always greater than $\frac{\dot{y}}{\dot{x}}$ when the curve, as in fig. 3, is convex to the axis; if a curve concave to the axis be drawn through the point E in the figure, another approximating fraction less than $\frac{\dot{y}}{\dot{x}}$ will approach $\frac{\dot{y}}{\dot{x}}$ from below the tangent, and $\frac{\dot{y}}{\dot{x}}$ is, in the strictest sense of the word, the *limit* which the fractions approximating from above and from below the tangent can never pass.

Thus another objection of Lagrange is removed. This objection is, that what in the fluxional or differential calculus are called limits, are not so, in the strict ancient meaning of the word limit.

Whatever be the magnitude of each of the quantities E F and F G = $\dot{x}$ and $\dot{y}$, the fraction $\frac{\dot{y}}{\dot{x}}$ is of the same value at the point E of the curve; the fluxions or differentials, therefore, are not zeroes, though Euler and others have endeavoured to show that they are zeroes only; but they are indeterminate quantities with a determinate ratio. They are, therefore, common algebraic quantities; and may be sometimes zeroes, sometimes infinite.

I have taken, as a function of x, $y = x^n$. But I might have taken another function of x. Perhaps it would have been better, because more general, if I had taken $y = f\,x$; $y' = f\,(x + i) = f\,x + p\,i + q\,i^2 + r\,i^3 +$ &c., i being an indeterminate quantity; the reasoning, however, would, with certain conditions, have been the same, but probably less intelligible to a learner. The conditions are, that from x to $x + i$ the curve turns its concavity to the same side, and that the ordinates continue to increase.

From what has been said, it is easy to teach the learner the formulas for tangents, quadratures, cubatures, complanations, and rectifications of lines, whether of single or of double curvature.

It is likewise so easy to teach him to calculate a few logarithms, by the series derived from the asymptotic spaces of the hyperbola, that I think the calculation and demonstration should always precede trigonometry. All this he may do without hearing the hyperbola even named.

From the 47th proposition of the first book, or from the 14th proposition of the second book, of Euclid, he may derive an algebraic equation to the circle; and from that equation he may, without a previous knowledge of the conic sections, obtain equations to the ellipse, hyperbola, and parabola; he may, indeed, obtain an equation to the parabola without any knowledge of an equation to the circle. Thus he would have various subjects to which he might apply the fluxional calculus.

Might not those authors who in algebra treat of variable quantities, subjoin the rate of variation, and thus make fluxions a part of common algebra?

Article III.

On the Methods of detecting Carbureted Hydrogen Gas in Coal-Mines without setting it on Fire. By John B. Longmire.

It is not generally known that miners are in possession of means which, if properly used, will detect the presence of carbureted hydrogen, or inflammable gas, in coal-mines, before it can do them any injury. For this purpose they make use of the smell and the sight simply, and the changes induced on the *top* of a burning candle.

A tabular body of mist-like gaseous matter is sometimes observed under the roof of the mine, and every motion communicated to the air gives it a corresponding one. Miners have reasons, deduced from dear-bought experience, to suspect the inflammability of a body presenting this appearance. Such a body, however, does not always possess this property: for it may consist of a mixture of carbureted hydrogen gas and the vapour of water, which on entering the mine ascends to the roof, and remains there till it is displaced by a current of air, or accidentally set on fire by the miners; or it may be the tobacco-smoke of the hewers and the vapour of water which proceeds from the breath and bodies of the hewers, and the persons employed to remove the coals; and which, along with the tobacco-smoke, is dispersed through the air by the people at work, but which during their absence ascends to the roof, and collects into a stratum of vapour, so similar in appearance to the mixture of gas and vapour, as not to be distinguished from it but by the most experienced eye. In this view of the subject, this method is fallible; and as the carbureted hydrogen gas is very often pure, or unmixed with any other gaseous matter, its body is not visible. Hence in either case a miner who trusts to his sight only will be very often deceived.

A miner who uses the smell leaves his candle in a working at a sufficient distance from its forehead, and advances slowly towards it; he then traverses the working in many directions; and if he finds no smell indicative of the presence of the inflammable gas, he brings his candle to the forehead. But pure carbureted hydrogen gas is odourless, and of course when its presence can be known by the smell, it must be mixed with other kinds of gaseous matter. It is said sometimes to have a sulphureous odour. I have not, however, been able to detect it by this; but I have often done so by an empyreumatic odour, which probably proceeds from a subtile oily substance that sometimes leaves the coal along with the gas. As carbureted hydrogen gas in coal-mines is often pure, its existence in them cannot always be known by the smell. Neither the sight nor the smell, therefore, are to be implicitly relied on; but they are often very useful; for a man may be travelling through a mine hastily, or inattentive to the state of the circulating air, with his head in a mixture of inflammable and common air, and his hands and candle in the common air only, and be advised by the sight or smell of the presence of the inflammable air, before he accidentally elevates the candle and sets it on fire.

But the miner can place the greatest reliance on certain changes induced by the inflammable air on the top of a burning candle. The miner's term of the candle-top will not be understood without an explanation. It is that faintly luminous yellowish-brown body which closely surrounds the flame, and which is largest above it, and at last passes into the smoke. This body is rather obscurely seen round the flame of a candle in the night, in a room not much illuminated by any other body; but it may be distinctly seen by using the following means:—Hold the candle in one hand at about a foot or 18 inches from the eye, interpose the other hand between the flame and the eye at about one inch from the candle, and let only a very small portion of the upper part of the flame be seen. By this arrangement the vivid light emanating from the flame will be intercepted, and the body in question observed for one-eighth to a quarter of an inch, and sometimes higher, above the flame. By changing the position of the hand, so that a small portion of the flame round either side may be seen, this body will have, from the top to the middle of the flame, a yellowish-brown colour, about one-sixteenth of an inch broad; below the middle, the same colour inclining a little to a purple; and downwards it gradually passes into an ultramarine blue, which is the colour of the flame at its bottom. At that place too, this body joins the flame. Such is that body which the miner calls the candle-top, as it appears when a candle is burning in pure atmospheric air; and as he uses only that part of it which is above the flame, his term of *top* is very applicable.

Now there are certain differences in the top of a candle between its burning in a mixture of atmospheric air and carbureted hydrogen gas, and in the air only. These differences are proportionate to the quantity of gas in the mixture, and point out to the miner when the quantity is such that a very little more would cause the instantaneous combustion of the whole body of gas. The alteration in the top consists of a change in colour, and of an increase in its dimensions. These changes become greater as the quantity of gas increases. The change of colour which is universally considered as a sure indication of the existence of carbureted hydrogen gas, and of course the most dangerous change, *is from yellowish-brown to greenish-blue*; especially when it is accompanied with a rapid assension of very small luminous bodies, generally called points, which appear to be propelled upwards through the flame and top with a very faint crackling noise. The precise dimensions of a dangerous top is not the same in all coal-pits, nor in every situation in the same pit, nor in every candle in the same situation: so that the top which might be considered very dangerous, in point of size, in one situation, is not so much so in another. Hence a great latitude is left for the miner's judgment and prudence; and if he be experienced in his candle symptoms, and well acquainted with local differences, he may, if necessary, venture much further in a mixture of airs, than a stranger to that particular situation with the same degree of judgment. I have seen the greenish-blue top one inch and a quarter long above the flame of the candle. Other miners say they have seen it longer; but others say the inflammable air has fired when it was shorter. With respect to this variation in the size, one general rule may be adopted, which is, not knowingly to get past the limits of certain experience, unless the object to be obtained is much greater than the risk run. The size of the top appears to me to depend in a great degree on the heat of the mine. I have examined the top in parts of a mine where the air was stagnant but pure, and I always found it greatest in the hottest part. In making these experiments I have often seen it three, sometimes four, and once five, inches above the flame, without any difference in the colour except in intensity, which increased with the size of the top. I once advanced towards the forehead of a working in which the air was still, with a candle having a deep yellowish-brown top, four inches long, till I observed longitudinal streaks of bluish-green were mixed with the yellowish-brown. I then began to retreat, being convinced that my situation was dangerous. I have often thought the moisture in the air of the mine affected the top; but I never met with proper situations to make so many experiments as would enable me to come to very accurate conclusions.

When a miner is about to use his candle he snuffs it, and then waits till the top of the wick is inflamed. As on such an occasion as this he generally carries his candle as near the ground as his arms will permit, he never raises his candle, but depresses his body till his head is nearly on a level with the candle, and probably he may kneel on one knee; he then, holding the candle in the manner before described, and having his eye steadily fixed on the top, rises slowly, till the candle is nearly as high as his face when he is standing upright. If he finds the top has a favourable appearance, he gently sinks his candle again, and advances a few yards; but if the top indicates the presence of the inflammable gas, he experiments more cautiously, and advances a less distance between every trial; if the quantity of gas appear to be increasing, he advances very slowly, with his eye constantly fixed on the top, and his body rather bent forward; and if the quantity be still increasing, and the appearance of the top rather alarming, he advances still more slowly, and pauses at every yard in length, to be more exact in his examination. And if the quantity of gas reach its maximum before he considers the top to be dangerous, he advances slowly to the end of his journey; but if it continue increasing, he will be forced to return.

Much steadiness is required in these trials; for sometimes the top suddenly assumes so alarming an appearance, that a person may be intimidated so much as to make a precipitate retreat; and in doing so sets the gas on fire, by bringing a quantity of it, or a *sop*, as the miner calls it, upon his candle, by his rapid movement. Before he tries, he should first consider what he ought to do in every probable exigency; so when the top suddenly appears dangerous, he will sink the candle slowly to the ground, and bend his body as much as he can at the same time, then turn about and retreat as slowly; avoiding, if possible, the direction that he advanced in during the dangerous part.

ARTICLE IV.

Population of the United States of America, and Territories thereof, agreeable with an actual Enumeration made according to Law, in the Year 1810.

Names of Districts and Territories.	Males of all ages.	Females of all ages.	Strangers.	Slaves.	Total in each district.
Maine	115,509	112,227	969	—	228,705
Massachusetts	229,742	235,561	6,737	—	472,040
New Hampshire	105,982	107,508	970	—	214,460
Vermont	109,951	107,194	750	—	217,895
Rhode Island	35,743	37,471	3,609	108	76,931
Connecticut	126,873	128,606	6,453	310	261,942
New York	474,281	444,418	25,333	15,017	959,049
New Jersey	115,357	111,511	7,843	10,851	245,562
Pennsylvania	401,566	385,238	22,492	795	810,091
Delaware	28,006	27,855	13,136	4,117	72,674
Maryland	120,210	114,907	33,927	111,502	380,546
Virginia	280,038	271,496	30,570	392,518	974,622
Ohio	119,657	109,204	1,899	—	230,760
Kentucky	168,805	156,492	1,713	80,561	406,511
North Carolina	188,662	187,778	10,266	168,824	555,500
East Tennessee	46,813	44,668	519	9,376	101,367
West Tennessee	64,950	59,444	807	35,159	160,360
South Carolina	109,547	104,649	4,554	196,365	415,115
Georgia	75,865	69,549	1,801	105,218	252,433
Territory of Orleans	18,940	15,871	7,585	34,660	76,556
——— Mississippi	12,850	10,274	240	17,088	40,352
——— Louisiana	9,387	7,838	607	3,011	20,845
——— Indiana	12,590	11,320	393	237	24,520
——— Illinois	6,380	5,121	613	168	12,282
——— Michigan	2,837	1,781	120	24	4,762
District of Columbia	8,130	7,949	2,549	5,395	24,023

Total of free inhabitants in the United States	5,781,443
——— Territories	131,268
Total of strangers	186,446
——— slaves	1,191,364
Grand total	7,240,521

Population of the principal Towns in the United States of America.

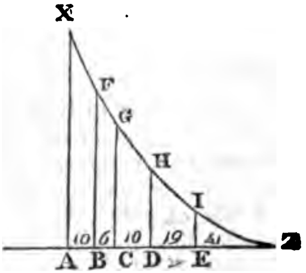
Boston	33,250	Philadelphia	53,722	75,096
Salem	12,613	East and West Northern Liberties of ditto	21,374	
Providence	10,071	Baltimore		35,583
Newport, Rhode Island	7,907	Washington county		18,730
Newhaven city, Connecticut	5,772	Charlestown		24,711
New York	96,373	New Orleans		17,242
Newark, New Jersey	8,008			

I am indebted for the following curious speculation on this subject to John Leslie, Esq. F.R.S.E. Professor of Mathematics in the University of Edinburgh.—T.

The population of America at the different periods of life may be nearly represented by the differences between the complimentary spaces of a parabola.

Thus, the population under ten years of age may be represented by the space A B F X, of which the boundary X F is a portion of a parabola, X Z, whose vertex is Z.

From the property of the curve, D H is as DZ^2. Therefore $DH \times DZ$ is as DZ^3; and therefore any trilinear area, as D H Z, being one-third of the rectangle $DH \times DZ$, is also as DZ^3. Therefore the area



A F is as $AZ^3 - BZ^3$ or as $86^3 - 76^3 = 636056 - 438976 = 197080$
B G $BZ^3 - CZ^3$ $76^3 - 70^3 = 438976 - 343000 = 95976$
C H $CZ^3 - DZ^3$ $70^3 - 60^3 = 343000 - 216000 = 127000$
D I $DZ^3 - EZ^3$ $60^3 - 41^3 = 216000 - 68921 = 147079$
E I Z $EZ^3 - O^3$ $41^3 - 0^{\circ} = 68921 - 0 = 68921$

If now we multiply these proportional areas by 10, and retain the nearest figure in the place of thousands, they will stand as in the second column below, while the gross return of population is as in the first.

	Aggregate Return of Males and Females.	Proportional Numbers on the Parabolic Theory.
Under 10 years of age ..	1,970,000	1,970,000
10—16	897,000	960,000
16—26	1,085,000	1,270,000
26—45	1,089,000	1,471,000
45 and upwards ..	691,000	689,000

It appears, therefore, that in the first and last periods the areas are very nearly proportional to the numbers alive at those periods. But in the second, third, and fourth, periods, these areas exceed the return; which excess increases from the earlier to the more advanced ages; so that the American population is increasing more rapidly than the parabolic theory supposed.

Article V.

Observations on the Objections that have been made to Mr. Prevost's Explanation of the Effect produced by a Mass of Ice on a Thermometer in the Focus of a Reflecting Mirror. By Mr. Richard Davenport.

(To Dr. Thomson.)

SIR,

Being on a visit in Edinburgh, I took advantage of the opportunity it gave me of attending the lectures of the Professor of Chemistry, and witnessing a series of admirably conducted experiments. In a lecture on caloric an experiment was made with a conical tube of polished metal, the result of which has been considered as tending to prove the actual radiation of positive cold. On considering the phenomenon here presented, it appeared to me that it might be accounted for exactly on the principle on which an apparent radiation of cold from a body of ice in the focus of a concave metallic mirror, on a thermometer in the focus of a similar mirror set opposite to it, has been explained; which explanation I had considered as satisfactory, and I had thought was generally admitted. However, on looking into Mr. Murray's Elements of Chemistry, I found he objected to Mr. Prevost's explanation, and considered the phenomenon as not yet accounted for. I found also in the New Edinburgh Encyclopædia, Mr. Murray's objections stated and admitted, as objections unanswered, and the whole question left in an undecided state.

Now as it appears to me that the phenomena both of the mirrors and the cone may be satisfactorily explained without supposing the actual existence and radiation of cold; and as the authorities I have mentioned are much looked up to; and as they leave an important question in philosophy in a state of indecision; if that explanation which I have said appears to my mind satisfactory, should on perusal appear so to you, you will perhaps not object to employ a page or two in the *Annals of Philosophy* by the admission of the following sketch. I am, Sir,

Your obedient humble servant,

Richard Davenport.

It is well known that if a body be placed in the principal focus of a concave parabolic reflecting metallic disc, and a hot body in the corresponding focus of a similar reflector standing at a distance from, but duly opposite to, the first, the radiating caloric of the hot body is reflected and condensed upon the body in the focus of the first reflector; proving that radiating caloric is reflected in like manner with rays of light. But if a comparatively cold body is substituted for the hot radiating body in the focus of the second reflector, a thermometer in the focus of the first indicates a temperature below that of adjacent bodies not in that focus. This experiment was, I believe, first brought forward by Mr. Pictet, and the phenomenon at first appeared to favour the hypothesis of the actual radiation of cold; but a well-known explanation of it has been given by Mr. Prevost. I have not been able to see this explanation in Mr. Prevost's own words, but I collect it as referred to in other books.

He considers that caloric is radiated from bodies at all temperatures. The thermometer, therefore, itself gives out heat by radiation. In consequence of this universal radiation, all bodies must also receive radiations. The thermometer, therefore, is receiving heat also; and while it receives an equivalent for what it gives out, no change of temperature is to be expected. But he adds, that the quantity radiated from all bodies is in proportion to the difference of their temperatures. Since, then, comparatively cold bodies emit comparatively weak radiations, when ice is placed in the focus of the second mirror the thermometer receives less than it emits, and thus loses in temperature.

"But," says the writer of the article in the Encyclopædia, "this explanation is totally inconsistent with the most obvious facts attending the cooling of bodies;" for, he continues, "it is evident that on this hypothesis a hot body ought to cool more slowly when it is placed near a large body of inferior temperature than when near a small one; because in the former case it must receive more calorific emanations than in the latter."

This is precisely what I deny, notwithstanding the deference I feel for the opinions of the writer of this article, as well as for those of Mr. Murray, whose objections follow.

The article continues, "It is also very properly objected by Mr. Murray, that of different surfaces which at a given temperature radiate different quantities of caloric, that which radiates least must be least powerful in returning caloric to the thermometer, and must therefore have least effect in counteracting the reduction of its temperature." It then goes on to quote Mr. Murray as saying, that if Mr. Prevost's explanation were just, a blackened surface (which it is proved by Mr. Leslie's experiments radiates the largest quantity of caloric) ought to produce the least cooling effect on the thermometer, and a metallic surface (which radiates the least) ought to produce the greatest."

I shall beg leave to answer this objection with the preceding one.

The Encyclopædia continues, "Rejecting then this explanation of Mr. Prevost's, we shall proceed to consider that of Pictet;" to which also they object, and therefore leave the phenomena entirely unaccounted for.

The writer of the article, however, does not infer from this phenomenon the absolute existence of a frigorific principle, but only argues "that the objections against Mr. Prevost's and Mr.

Pictet's explanation of the indication of decrease of temperature in the focus of the reflector, apply with equal force in cases of cooling by direct radiation and absorption;" which is certainly true.

I will now describe also the experiment I have above alluded to with the metallic cone.

A metallic conical tube of about 18 inches in length, whose respective apertures were one inch and five inches (the interior being polished so as to reflect powerfully and radiate little), being supported in a horizontal position, one ball of a differential thermometer was presented to the larger aperture, while a bottle of ice was presented at the smaller aperture. The liquor in the tube of the thermometer rose very little towards the bulb nearest the aperture, slightly indicating a lower temperature. The relative situations of the thermometer and the ice bottle being reversed, (*i.e.* the ice carried to the large end, and the bulb of the thermometer placed before the smaller end,) the liquor rose much faster towards the bulb, indicating in this position a much more rapid decrease of temperature.

In this case it was argued that since the rays admitted at the wide opening were brought to a focus at the smaller one, if the thermometer at the wide end radiated caloric, its rays must be lost upon the ice in the focus, and it ought to indicate a loss of temperature; which was not the case, or only in a very small degree: but that when transposed, as this was the only focus (the experiment in that respect differing from that of the mirrors), the loss of temperature which it so rapidly indicated could arise only from a collection of actual cold rays in the focus; and that this experiment constituted another objection to Mr. Prevost's theory.

This objection I shall endeavour to remove with the rest.

It will be convenient first to consider in what way a focus of rays (whether of light, or of heat, or of cold,) is produced.

Every point of a *luminous* body emits rays, proceeding in every direction in straight lines diverging from that point at all angles. Different parts of the same luminous body emit rays which cross each other at all angles; but which, after a few reflections, are absorbed, or dissipated into boundless space. But since every body possesses free caloric, more or less, and every point of these emits rays of caloric, as the points of luminous bodies do those of light, it follows that every body is surrounded on all sides by radiating and by receiving points; and may therefore be considered as surrounded by a sphere, from every point of which rays proceed and fall on it: and since the intensity is inversely as the squares of the distances, if the temperature of the parts of which the whole imaginary sphere consists is uniform, it receives equal quantities on all sides, although some of the surrounding bodies are much nearer than others; because the quantity of surface of distant bodies, intercepted by the intervening near bodies, is conversely in the same ratio; *i. e.* as the squares of the distances. Thus if a circular screen, whose area we will suppose equal to 36 square inches, be held at the distance of one foot from the thermometer, and the wall of the room be at the distance of eight feet from the same, the screen intercepts from the thermometer an area of 2304 square inches; but the intensity of the radiation from the wall, which is at eight times the distance of the screen, is to the intensity of the radiation from the screen only as one to 64, or as 36 to 2304; and thus the difference in quantity of radiating surface is compensated by the difference in intensity.

Now by mere reflection the actual number of rays, or the intensity of any given number, cannot be either increased or diminished. The intensity (whether of light or heat) is increased in the focus solely by the direction to one point of a number of rays subtracted by a change of direction from other points on which they would have fallen in their natural course; for it is self-evident that whatever radiation is superadded by mere reflection on one spot, must be subtracted from some other; and if the sensible heat is increased in the focus, it must be diminished somewhere else.

Some kinds of surfaces receive and emit heat by radiation with great facility; others reflect more and radiate less; as Mr. Leslie has shown. Mr. Leslie has also shown that the powers of receiving and giving out heat are in the same surface equal; and also that its powers of radiation and reflection are in inverse ratios. This seem also demonstrable from all usual appearances; for if any body could radiate either more or less than it receives, or reflecting a part only of what falls on it, did not give out by radiation a sum equal to that which enters, not only would adjacent bodies be affected by it, but the sensible heat of the body itself must continue to increase or diminish, without apparent limits: but it is not found that the temperatures of adjacent or remote bodies differ from each other by mere position, where there is no source of heat or active cause of increase or diminution of the actual quantity of free caloric; and since different surfaces of the same body radiate or reflect one more than another, if the emission and intromission by radiation were not equal in the same surface, or if the reflecting surface did not radiate all which it does not reflect, bodies exposed near one surface must be affected differently from those on the other. Thus if a flat plate of metal were polished on one side, and blackened on the other, two bodies of equal temperature with the plate must receive different temperatures from it; still more would a body in the principal focus of a reflecting mirror be affected; for there is an actual accumulation of reflected rays upon it. Yet a body placed in or out of the focus, or even before the concave or behind the convex side of the mirror, suffers neither increase nor decrease of temperature, neither is it affected by the proximity or removal of either the plane reflector or the mirror. Where is the compensation? In the case of the plane reflecting plate, the radiation of nearly one half of the surrounding sphere is intercepted, viz. all behind the plate; but the same quantity of radiation from the hemisphere in front of the plate which would have (which has, indeed,) passed the object, is reflected on it; and so much as the imperfection of the reflecting power of the plate suffers it to absorb, is radiated by it in the same direction. Here, then, is the compensation; and here I think Mr. Murray's objection fails. A blackened surface radiates much, it is true; but it intercepts an equal volume of radiation or reflection from behind. A polished surface radiates less, but it reflects as much as it fails to radiate. So when the writer in the Encyclopædia says "a hot body ought to cool more slowly when near a large body of inferior temperature than when near a small one," he forgets that this large body intercepts an equal volume from beyond it, and therefore the hot body so placed ought *not* "to cool more rapidly." He says the *reverse* is the case; but I apprehend it would be found, by careful experiment, that *if* the adjacent body be of the same temperature as those whose rays it intercepts (the screen, for instance, of the same temperature as the wall), the *reverse* will *not* be the case, but the time of cooling will be the *same*. (N. B. Dr. Wells's lately detailed interesting experiments on the formation of dew, in consequence of the loss of heat by uncompensated radiation, will be found to be in perfect conformity to this view of the subject.)

Then in the case of the concave reflector it is true that one hemisphere of converging rays is intercepted from behind, and only a cylinder of parallel rays in front, brought to the focus by reflection; yet this is a compensation: for the intensity of the rays so brought to the focus is equal to that of the converging rays which would have passed in the same direction through the space occupied by the disc, as might be easily shown by the general laws of radiation and reflection. When a second mirror faces the first, all this cylinder of rays is intercepted; but then all the rays which pass through its focus *to* its face are reflected in the direction of the intercepted cylinder, and a cylinder is formed of reflected rays, similar to, and in lieu of, the intercepted one. The whole cylindric space comprised between the two mirrors now consists of two sets of rays proceeding in exactly opposite directions from and to both the foci.

Let us now place a hot body in the focus of the second reflector. This last cylinder now consists of extraordinary calorific rays; and of course a body (as a thermometer) in the focus of the first mirror receives superabundant heat. Substitute an extraordinary cold body; then, as all the rays which would have passed through this focus, and would have constituted the second cylinder, are now absorbed by a body which has very little to radiate, the radiation of the thermometer towards the first mirror is uncompensated; and it indicates decrease of temperature.

In Mr. Pictet's detail of the experiment it was observed that when the temperature of the ice was lowered by the addition of nitrous acid, the thermometer sunk several degrees lower than before. The theory proceeds here with perfect regularity. Ice at + 32 radiates something towards the compensation: at − 20, still something, though much less; and therefore the small compensation was again reduced by the refrigorating mixture.

The comparing of the reflection of heat to that of light (though they are in many respects analogous) is apt to lead into some error, because in the latter case the reflector is presented in a particular direction towards a particular source (as to the sun, or to an aperture admitting the rays); but the rays of heat are received equally from every point of the surrounding sphere: but if we bear this difference in mind, the analogy between them will afford an evident illustration of the theory. Vision is merely the sensation of rays of light on the retina of the eye. We see a white or coloured object by means only of light reflected by it. Black, we say, reflects no light; yet we see a black object. If the blackness of the surface were really perfect, and reflected no light, we could not (strictly speaking) see it at all; we perceive or distinguish it by its interception of other forms and objects; but we can also magnify it by dioptric lenses or mirrors. To put the comparison in a still stronger view, suppose a white object (as a paper box blackened within), and a small hole cut in one of its surfaces, we say we see that hole. Do we *see* darkness? Through a convex lens, or by a concave mirror, or by the intervention of two mirrors (placed exactly as in Pictet's experiment, the eye being in the place of the thermometer), we may magnify that dark hole. Do we say darkness radiates? And where is the difference between the two cases; the one, where the ice in the focus of the second mirror lowers a thermometer in that of the first; the other, where a dark spot in a similar situation gives the sensation of a magnified dark spot on the retina of the eye? A perfect analogy holds here, and applies to all the cases, and removes at once (as it appears to me) all the objections made to Mr. Prevost's theory. It is true that in the quotations I have met with of Mr. Prevost's explanation he has not expressly described the *interception* by means of the ice, of radiation *aliunde*; but I think it follows by necessary inference that he had it in his mind as a part of his theory, and meant to be so understood. I have said before that his own words I have not met with.

I have now to answer the supposed objection arising from the experiment with the metallic tube; but if the explanations above given of the several phenomena are satisfactory, I have only to trace the analogy between them and this new one.

I think we are in possession of facts that warrant the following conclusions:—

1. That all bodies receiving and admitting heat by their surfaces emit by radiation an equal quantity; and that as much of what falls on them as they cannot so admit, they reflect.

2. That when the radiation of any body is not compensated by counter radiation, its temperature must decrease.

3. That the proximity or the form either of radiating or reflecting surfaces produces no difference in the quantity or intensity of the rays received by any given point, provided there is no active cause

of increase or decrease of the actual quantity of free caloric in the surrounding sphere; but that where there is such cause, such given point will be affected by the form and nature of the reflecting surfaces which change the direction of rays that fall on, or proceed from, the point where such active cause exists.

4. That where heat is increased by more reflection on one spot, it must be diminished somewhere else, and *vice versâ.*

5. That when any body whose temperature (arising from extraneous causes) varies from that of others in its neighbourhood, if hotter, it affects a body placed in any given point, by throwing on it a more intense radiation than that which it gives out; if colder, by intercepting and absorbing rays of free caloric from some part of the sphere by which it is surrounded, and thereby depriving it of its due and usual compensation.

If, then, by means of the conical tube, or by any other means, a thermometer indicates decrease of temperature from its relative position with that of a cold body, we have only to see where and how are radiations that would have fallen on the thermometer intercepted by that cold body.

The mechanical form of the polished interior of this tube will effect this according to the known laws of optics. The effect of this reflection differs from that of the mirrors; for instead of a single focus at one point, there is an indefinite number of foci all along the axis. The incident rays which are parallel to the axis are reflected in the form of cones, and cross in the axis: however, the largest hollow cylinder of incident rays will form the focus of greatest intensity, and a multitude of other rays will be brought by complex reflections from the polished interior to the same spot. The tube is of course a *truncated* cone; for the smaller end has a diameter. The principal focus is not at the apex of the cone, but short of it; and we suppose the thermometer to be placed at this spot near the aperture. I need not trespass more on you by specifying all the circumstances of the situation; but it is easy to see that a large portion of the rays of the surrounding sphere is intercepted from this point by the tube, and reflected from its exterior surface in foreign directions. The loss of these rays is compensated by the concentration of the cylinder of rays brought to this point by reflection from within the tube, else the thermometer in this place must indicate decrease of temperature without the presence of the cold body, contrary to the rule No. 3. But when the bottle of ice is brought to the wide aperture, all this cylinder of radiation is intercepted and absorbed, and the radiation from the thermometer is uncompensated. A hot body in the place of the ice would have its radiation condensed on the thermometer. A visible object would be magnified in an eye placed in the room of the thermometer, and a dark hole in a white surface would have its diameter apparently increased; and thus a perfect analogy subsists between this experiment and those of Mr. Pictet.

Mr. Pictet's theory appears also capable of accounting for the phenomena of his experiment, but appears to have been misunderstood. Still it involves some strong improbabilities; while that of Prevost, if we only admit certain analogies (some of which we know exist) between the radiation of heat and that of light, is even demonstrable. I was about to add a comparison between these two theories; but I have already made my letter much longer than I intended.

ARTICLE VI.

On the Cerebellum. By W. Elford Leach, M.D.

(To Dr. Thomson.)

MY DEAR SIR,

In the last number of your *Annals of Philosophy* Mr. A. Walker claims the merit of being the discoverer of what Dr. Cross has stated respecting the structure of the spinal marrow and the use of the cerebellum. He has at the same time answered my letter to Dr. Cross, and has stated that the work of Gall and Spurzheim "actually contains no such statements" as those to which I alluded.

Permit me, Sir, to assure you that the letter from Hufeland to Portal contains precisely the same opinion respecting the use of the cerebellum as that given by Mr. Alexander Walker and by Dr. Cross.*

When I perceived Dr Cross's observations on the anatomical structure of the spinal mass of nerves, I recollected that in the work of Gall and Spurzheim the same statements were given, not as existing in nature, but as erroneous suppositions. In the folio edition of their Anatomie et Physiologie du Systême Nerveux en général, vol. i. p. 35 and 40, the following statements may be found:—"Bartolin says that the spinal marrow is composed of four fibrous cords;"—"Soemmerring maintains the opinion that the spinal marrow is composed of four cords."†

My object in answering Dr. Cross was merely to show that although his opinion might have been original, yet that the same opinion had been entertained by preceding writers. Since writing that answer, I have carefully examined the structure of the spinal mass of nerves; and I most certainly agree with Drs. Gall and Spurzheim in maintaining that the spinal mass of nerves does not consist of "four columns," and that Mr. A. Walker merely participates in an error common to older writers in maintaining this opinion.

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* Willis likewise considered the cerebellum as the source of voluntary power.

† Highmore even goes farther: he pretends that the spinal marrow is formed of eight little cords.

It appears to me that it is of much greater consequence to disprove these statements, than to dispute about their origin. I shall, therefore, in the first place, examine the statements relating to the structure of the spinal marrow, and shall then produce some objections relative to the functions of the cerebellum as stated by Willis and by Mr. A. Walker.

It is said that the spinal marrow is composed of four longitudinal divisions, two larger, anterior, which may be traced into the cerebrum; and two smaller, posterior, which may be traced into the cerebellum. This statement is erroneous in two respects: 1. There are not four divisions; there exist only two fissures, one at the anterior part of the spinal marrow, the other at the hinder part; but there are no lateral fissures. Such are the statements of Gall and Spurzheim; and my recent examinations have convinced me of their truth. Gall and Spurzheim have shown that the spinal mass of nerves is composed of a series of as many swellings as there are hairs of nerves, and they have pointed out that the anterior fissure* is less deep than the posterior, and that the posterior nerves are more considerable than the anterior. It is, therefore, erroneous to say that of the four divisions the anterior are larger, and the posterior smaller.

Mr. A. Walker considers the anterior fasciculi as ascending, and the two posterior as descending. Gall and Spurzheim have shown that the brain and cerebellum cannot be considered as the continuation of the spinal marrow, any more than the spinal marrow can that of the brain and cerebellum. There are examples of monstrous fœtuses having only the lower parts of the spinal marrow, and yet the posterior and anterior (or, according to Mr. A. Walker, the ascending and descending,) fasciculi have been found to exist. It is, therefore, evident, that they cannot be considered as descending from the cerebellum. Gall and Spurzheim have also shown that not only the cerebellum, but also the hinder parts of the brain, are brought in communication with posterior parts of the spinal mass of nerves, and that only a small fasciculus of the hinder part of the spinal mass of nerves belongs to the cerebellum.

The physiological statements of Mr. A. Walker are not more accurate than his anatomical ones; they are merely suppositions, and at variance with nature. The cerebellum he considers as the organ of volition, because it is placed opposite to the face, which contains the chief organs of sense. The expression *opposite* as to function has no specific meaning. It cannot indicate a counteraction; if this were the case, the ears, which are situated opposite to one another, should act in opposition to each other.

I must repeat Mr. A. Walker's supposition in his own words:—"From the peculiar *opposition* which subsists between the situation of the face and cerebellum, we are entitled to expect a similar *opposition* in their functions. As the face, therefore, occupied by the chief organs of sense, is the seat of sensation, so we might expect *the cerebellum* to be *the organ of volition*. This supposition receives additional force from the consideration that, as the organs of sense and the cerebellum are the first and the last portions of the nervous system; so sensation and volition are the first and the last of its functions. But this supposition is completely confirmed when we recollect that the degrees of voluntary power *always* bear a close analogy to the various magnitudes of the cerebellum."

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* Anat. et Phys. &c. pl. ii. fig. 6.

If we desire to listen to, or to look at, any object, the muscles support the action of seeing and of hearing: hence sensation and voluntary motion are not in opposition. It is also quite erroneous to suppose that only the posterior nerves of the spinal mass go to the muscles, and the anterior to the organs of sensation; and it is incorrect to believe that almost all the parts of the body have filaments of ascending and filaments of descending impressions. The two roots of nerves of each half of the spinal marrow, namely, the anterior and posterior, go to different parts of the body; and it is erroneous to admit that filaments of the anterior and posterior fasciculi of the spinal mass of nerves go to almost every part: the muscles and skin of the back receive their nerves from the posterior roots, whilst the muscles and skin of the abdomen receive theirs from the anterior roots, and yet the fore and back parts of the body have sensation and voluntary motion.

Mr. Alexander Walker, in considering the cerebellum in opposition to the face has neglected to take any notice of the brain. Does he therefore consider that part to have nothing to do with sensation and volition?

The degree of voluntary power bears no proportion with the size of the cerebellum.

Gall and Spurzheim have shown that the cerebellum in children is much smaller in proportion than in the adult, and yet children have more of muscular agility than adults.

The large shark described in the Philosophical Transactions for 1809 and 1813, an animal said to have the greatest locomotive power, has a remarkably minute cerebellum. See vol. an. 1809, pl. xix. *a*, and pl. xx. *a a*.

Swallows, which possess amazing locomotive powers, have a small cerebellum; and Cuvier has shown (Leçons d'Anatomie Comp. tom. ii. p. 153), that the proportions of the cerebellum and cerebrum in the bull and in man are absolutely the same, yet their degree of voluntary power is very different. It is true that fishes have a larger cerebellum in proportion to their brain, and that they possess a very strong locomotive power; and the rongeurs have likewise a very large cerebellum; and many of them, as the hare, squirrel, &c. possess a great locomotive power; but this is no general principle.

I shall here subjoin a few comparisons of the cerebellum and brain extracted from Cuvier (loc. cit.), and show that there is really no proportion between the degree of voluntary motion and the size of this organ.

Man and the ox	1—9
Dog	1—8
Horse and wild boar	1—7
Hare and cat	1—6
Sheep	1—5
Mole	1—4⅛
Rat	1—3¼
Beaver	1—3
House mouse (*mus musculus*)	1—2

Mr. A. Walker has further observed, that "Dr. Spurzheim has most certainly erred in denominating the cerebellum the organ of amativeness." I have not time to give the facts which have led Drs. Gall and Spurzheim to hold this opinion, and must therefore refer to Dr. Spurzheim's work on the Anatomy and Physiology of the Brain, where the statements are fully detailed.

I am, dear Sir,
Your obedient servant,
Wm. Elford Leach.

Article VII.

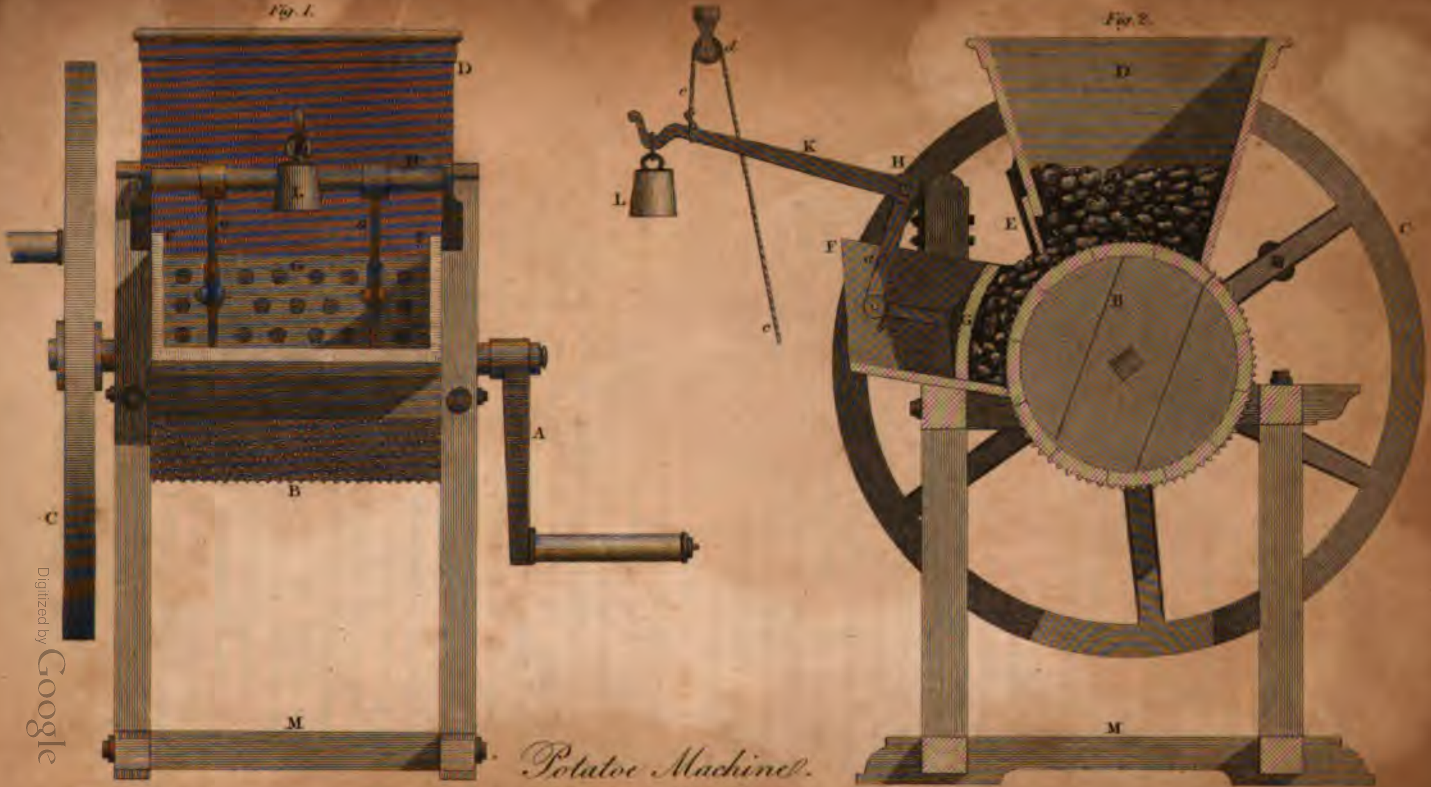
On Pyrophorus, and the making of Potassium. By Dr. Hamel.

In the appendix to vol. iv. of Priestley's Experiments and Observations relative to various Branches of Natural Philosophy, p. 481, mention is made of the following experiment of Mr. Wm. Bewly: — "I mixed equal parts of sal-tartar and vegetable or animal coal, or sometimes three parts of the former with two of the latter, and calcined them in the usual manner (for making Homberg's pyrophorus). This composition, on being exposed to the air, generally kindled in the space of half a minute or a minute. It did not burn with so much vivacity as the *vitriolic* pyrophori, as it contained no sulphur. I shall hereafter denominate this the *alkaline* pyrophorus. It differs in no one circumstance from M. du Suvigny's *neutral* pyrophori, except in its *not* containing that very principle to which he ascribes their accension. Lest it might be suspected that the salt of tartar which I employed might accidentally contain vitriolic tartar, or vitriolic acid, I repeated the experiments with tartar calcined by myself, as well as with nitre fixed or alkalized by deflagration with charcoal and with iron filings; but in all these cases the event was the same. It is rather surprising that this *alkaline* pyrophorus should not have been discovered before, as I have more than once, since I first noticed it, on preparing the Prussian alkali, seen the lower part of it take fire soon after its having been turned out of the crucible and bruised, even when the matter had not been covered with sand."

If these experiments of Mr. Bewly be correct, they evidently show that the inflammability of Homberg's pyrophorus depends (at least partly) on the mixture of potassium (and perhaps also alumina) with fine powder of charcoal, and that the common theory, according to which the inflammability of pyrophorus depends on the sulphuret of potash, imbibing moisture from the atmosphere, and by that means setting at liberty heat enough to inflame the charcoal mixed with it, is insufficient. It appears that Mr. Bewly followed nearly the same method in order to make his pyrophorus, as Bucholz, Tromsdorf, and Curaudau, did with a view to obtain potassium; and if Mr. Bewly had happened to introduce into the vessel containing the heated mixture a cold metallic body (as Curaudau did in order to collect the potassium which is formed during the process, and driven off in vapours), he would have seen more than 30 years ago one of those bodies by the discovery of which Sir Humphry Davy's name is now immortalized. As Mr. Bewly's experiment was inconsistent with the then prevailing theory, there was hardly any attention paid to it, though it was published as long ago as the year 1779; at least I do not recollect that it was repeated. I found that an alloy of *potassium with iron* is a pyrophorus also. When I prepared potassium the first time, in the way which Gay-Lussac and Thenard describe, I did not know the property of this alloy; and having cut off that part of the barrel containing the potassium, I began to scrape it out, holding the barrel over a glass bason containing petroleum. I had poured out my whole stock of petroleum, perhaps two pounds, into this bason, that I might be able to immerse into it the whole piece of the barrel, in order not to lose any potassium. After I had scraped out that portion occupying the centre of the barrel, which is the purest, I came to that in immediate contact with the inner surface of the barrel itself, which is generally alloyed with iron; and a small piece of this alloy, being just scraped off, and having therefore its surface quite free from oxide, in falling down took fire, by coming in contact with the air, and lighted the petroleum contained in the glass bason, which cracked, and, the burning fluid flowing over my clothes, placed me rather in a dangerous situation. I afterwards have seen the same effect frequently. I think this pyrophorus of potassium and *iron* leaves no doubt that there may be one consisting of potassium and charcoal merely; and this will throw light on the nature of Homberg's pyrophorus.

In order to obtain a potassium free from iron, I several times followed a method which is very simple, and on that account deserves perhaps to be known, though it did not yield me quite so large a quantity as the apparatus of Gay-Lussac and Thenard; which, however, does not yield so good potassium, and comes much more expensive. I have made a good deal of potassium, and found, 1. That the chief secret to obtain it in considerable quantity is to use a very strong fire, in order to drive every particle of it over. 2. To prevent the barrel from melting in such a high degree of heat a good tube is wanted. 3. The barrel, notwithstanding these precautions, will generally serve only once. 4. For this reason all the contrivances, where there are different parts combined by grinding them together, or by means of screws, come too expensive, as they are lost after having been used once. I used to make my potassium in old gun-barrels, which were bent (like Gay-Lussac's and Thenard's) in two places, and had no other piece added to them. The middle part of this barrel is placed almost horizontally (only a little elevated towards the open end) through a cylindrical furnace, and contains the iron turnings; the thicker end, which projects on one side from the furnace, is bent upwards, and contains the potash. It can easily be tightened by screwing in the breach, and putting a little clay into the touchhole. To the open end, which projects on the other side from the furnace, and is bent downwards, at first I adapted, as the French chemists did, a receiver made of iron, from which a tube descended into a vessel with quicksilver. I likewise, according to their prescription, screwed into the upper part, containing the potash, an iron tube, the end of which was placed under mercury also; but I found this to be a very troublesome and expensive apparatus, as I could not use it a second time; nor did I always observe the gas coming out through the quicksilver, as it would find its way out somewhere else, either where the receiver was adapted, or where the pipes were screwed in. Finding, therefore, these pipes quite useless, I closed the upper end of the barrel, after the introduction of the potash, entirely by screwing in the breech and putting some clay into the touchhole, and instead of adapting to the lower end any receiver, I put this end, open as it is, into a small iron bason, containing a little vegetable oil previously well heated, in order to drive off the watery particles. (Petroleum could not be used for this purpose, as it would be set on fire by the gas, and even by the heat of the furnace.) When the formation of potassium was going on, one bubble after the other of hydrogen gas holding potassium in solution rose through the oil, and coming in contact with the air took fire, like phosphoric hydrogen gas, but with a more considerable explosion. The purest part of the potassium run down in drops into the oil, in which it rose to the surface, from whence I took it with a spoon, and put it into petroleum. The potassium so obtained did not contain any iron, but was of a very bright lustre, much like silver, whereas that collected in the barrel is generally alloyed with iron, and of a bluish colour, more like lead. Living near an iron-

Plate XXXIV



Potatoe Machine.

Engraved for Dr Thomson's Annals for Baldwin, Cradock & Joy Paternoster Row May 1 1815.

foundery, I had the opportunity of making my potassium there, as I could apply the blast from the smelting furnace to my apparatus, and then I got it in considerable quantity. I generally laid two barrels parallel to each other through the furnace, and thus got once nearly two ounces; but I repeat, that without being able to give a very high degree of heat, it is not worth while going about making potassium, as the quantity obtained will be very trifling. For a lute, to cover that part of the barrel which comes in immediate contact with the fire, I tried mixtures of pure clay with sand in different proportions; but none of them answered my purpose so well as the clay by itself, which is found near Moscow, and used for crucibles all over Russia; it is known in commerce by the name of Moscow clay. Using this clay, I could give almost any degree of heat that can be procured from charcoal (which I always used, we having no coal in St. Petersburgh), without melting the barrel. The open ends of the barrels were placed under oil in one small bason; and after the operation was finished, I simply corked them, in order to prevent the air getting in, till I had cut off the under parts of the barrels, in order to obtain the potassium also which was collected there. This latter is, as I have said before, always less fusible, and alloyed with iron, whereas the other, which does not remain for any time in contact with the iron barrel, runs down into the oil, which absorbs the oxide, which might be on its surface, forming a soap with it. This potassium was the purest I ever obtained. I used to keep it in small glass tubes, in which I pressed it close towards the inside of the glass, and the tubes being hermetically sealed.

I am, dear Sir, yours most faithfully,

Glasgow, March 15, 1815. J. Hamel, M.D.

Article VIII.

Description of a Machine for the speedy Separation and Manufacture of Farina or Flour from Potatoes, and for various Observations relative to the Necessity of encouraging an extended Growth of Potatoes, and for their Applications, in various States, to make Sea Biscuits, Bread, Pastry, &c. By John Whately, Esq. of Cork. With a Plate.*

It is naturally the first object, and forms the primary duty of a well-regulated state, to provide its members with a sufficient quantity of good food. The greater are the advantages, and the more independent is the situation, of that country which can furnish within itself a full and ample supply of the means of subsistence to its inhabitants. We have long ceased in this country to feel the benefits arising from such a state of independence, and we have during the last 10 or 15 years incurred a very large expenditure in the purchase of foreign corn. This subject has engaged much of my attention; and I have endeavoured to devise a plan by which the agriculture of this country may be improved, a great expenditure saved, and a considerable reduction of prices effected, by the introduction into bread of an article of known nutritious properties derived from the potatoe. This substance is the farina or flour of that valuable root, which I have tried with advantage to a very great extent, having previously gone through a long course of experiments on making bread, biscuit, and pastry, from mixtures of raw potatoes, and boiled potatoes and farina, with different proportions of flour, &c.

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* From the Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce. The lesser gold medal was voted to Mr. Whately for this invention.

It is well known that poor light soils, unfit for the culture of wheat, and from which only crops of barley or oats have been produced, and of which, in many situations, there are large tracts of land altogether uncultivated, will produce potatoes of excellent quality: two circumstances have, however, hitherto prevented their culture to the desirable extent, namely, the great expense of their carriage in a raw state from the interior parts of the country to a proper market, and their not keeping well more than six months after the crop is gathered.

These inconveniences I have obviated by the invention of a machine which is now at the Society's house, by which one individual can grind down 15 hundred weight, or 1700 lbs. net of potatoes, into soft pulp in one day, yielding about two hundred weight of farina or flour when dried.

By calculation, the power of a single horse will be equal to the grinding of 22 tons of potatoes per week; the machine is capable of being applied to any power required. The farina or flour has been known to keep good 17 years; and it may be afforded so as to yield a fair profit to the farmer and the manufacturer at 3*d.* per lb. The present price of wheaten-flour is about 5½*d.*

It would be tedious now to enter into a long detail of this business by letter; but my son is in London; and if he is permitted to attend a Committee of the Society, and to give proofs of the trials I have made, by exhibiting specimens of the bread, flour, and biscuits, and by furnishing such other particulars as may be required, I trust they will perceive the great benefits that may result from this article, and particularly if my communications should merit the sanction of the Society, and be honoured with their approbation.

Remarks from Mr. Whately on Potatoes.

It may not be generally known, but it is capable of the most satisfactory proof, that the same quantity of land will yield above one half more of farina or flour where potatoes are cultivated, than if the same land was applied to the production of flour from wheat.

I have proved from experiment, that 2619 lbs. of pure farina or flour may be produced from an acre of land planted with potatoes, and only 1660 lbs. of flour from an acre of wheat. It will therefore be obvious that, if we can apply this great excess to the same purposes as the flour of wheat, the advantages arising from it will be of the highest importance to the community. We now require from foreign countries about 500,000 quarters of wheat annually, for which we incur an expenditure of about two millions sterling, in ordinary years, and which has increased in years of scarcity to upwards of seven millions sterling, and there seems no probability that, under the continuance of the present system of cultivation, we shall ever arrive at the period when we can supply our own wants, and feel independent of foreign aid for one of the first articles of subsistence.

We have had for the last ten years not only a course of favourable seasons, but the farmer has had in the greater part of that period the benefit of high prices to stimulate his exertions to the greatest possible production of corn; and if they have failed, under such favourable circumstances to satisfy our wants, what evil may we not expect if the seasons should prove unpropitious, and the inconveniences of our situation aggravated by scarcity? I admit that the prospect of supplies from foreign countries may be great; but we should recollect, that only a few months have elapsed since the ports of Europe and America were shut against us, or only open on a system of license, and that in a situation of dependance which no well-wisher to his country can be desirous to see recur, nor the possibility of which he will not feel anxious to prevent. This, then, is one of the advantages which will attend the consumption of potatoe flour in bread; the earth yields it so much more abundantly, that the very first year's cultivation of potatoes for that purpose, to the extent of only 25,000 acres, would relieve us from the necessity of any foreign importation, and instead of importing, we should speedily become an exporting country. Potatoes are found to possess such highly nutritive properties, that it seems hardly necessary to enter upon that subject. They constitute, it is well known, in their natural state, the principal food of the Irish, and they are daily forming a great part of our own. This farina or flour, which contains the substantial nourishing properties of the root, deprived of its watery particles, cannot but be highly nutritrious; the use of it, therefore, in bread, is not likely to be the subject of dispute, provided it is in that state palatable, and can be afforded at a price not exceeding the price of wheaten flour. That it possesses the first-mentioned property, I have only to refer to the bread produced to the Society, containing different proportions of this flour, any, and all of which will, I believe, be found as wholesome and agreeable as bread made wholly of wheaten flour.

In some respects it seems an improvement, as it keeps much longer, and it rises and becomes very light and agreeable when toasted. Upon the second question, I shall be able to show satisfactorily that the flour of potatoes can be produced at a cheaper price than the flour of wheat. All objection, therefore, to its use in bread, is not only removed, but I conceive the most important advantages will flow from its adoption. In the specimens before you one-fifth of the farina is the smallest proportion made use of, and two-thirds of farina and boiled potatoes together, when in bread, the largest; calculating, however, that no greater proportion than one-fifth should be generally introduced, the saving of wheaten flour alone, from that circumstance, would be immense. The population of Great Britain consists, according to the latest returns, of upwards of twelve millions and a half, and of this number it may be conjectured that about ten millions consume wheaten bread. Each individual's consumption has been considered equal to a quarter of wheat per annum. The introduction of potatoe flour, therefore, generally, would produce a saving of two millions of quarters of wheat per annum; and instead of being, as we now are, an importing nation, we should, after supplying our present deficiency, have a surplus of about 1,500,000 quarters for exportation, which, at the present permitted export price, under 54*s*. per quarter, would be equal in value to four millions sterling! That this surplus would not be obtained without some sacrifice of other crops, I am ready to admit; but when the increased productiveness of the potatoe is considered, when it is calculated that large tracts of land may be cultivated upon this system that would not be cultivated upon any other, and that the plan may be generally adopted, which prevails universally in Ireland, and has the recommendation of distinguished agriculturists in this country, of making the potatoe crop precede the crop of wheat, the sacrifices would upon the whole be few, and the advantages immense. I know that objections exist from the scarcity of manure, &c. but I am satisfied that many of them would yield in practice. The poor Irish contrive to manure the land, and to produce crops that serve to form the winter supply of the city of Dublin, in situations distant from cities, and where artificial manure is not to be had; lime, sand, or sea-weed, are all excellent, and are in many places easily procured; the burning of the surface in rough uncultivated land is almost sure to produce a large crop, and is generally one of the first, as it is found one of the best preparatives to cultivation. The manure from hogs has been stated to afford very great returns; and as it is almost essential to the economical pursuit of the business of manufacturing the potatoes into flour that many hogs should be kept, for the purpose of consuming the skins, refuse, &c. it is a manure that would be abundant in those places where the manufacture was introduced. Perhaps no situations would so strongly feel the benefit of this system as those which are remote from sea-ports, and distant from water-carriage. The ground is now necessarily employed in pasture, or it remains waste.

The expense of carriage is an insurmountable bar to the pursuit of agriculture. But let us now consider the situation of a farmer adopting this system. He chooses a remote part of the country, where, perhaps, cultivation did not before exist. He takes the land at probably one-fifth part of the rent which he would pay in more frequented spots, and he either invites the manufacturer of the farina to settle along with him, or he resolves to become a manufacturer himself. The first year he begins by burning and paring the surface of a part of his land, and by this preparation ensures an abundant crop. He sows the next year barley or oats on the land thus cleaned and prepared, which will serve as food for his cattle, and for the large stock of hogs that, as a manufacturer of farina, he must support; and he proceeds to bring a further part of his land under cultivation by the same means. The potatoes are manufactured on the farm; and if he could afford to send the potatoes seven miles to market in their original state, he can, without disadvantage to himself, convey them 50 in their manufactured state, as they are then reduced to about one-seventh or eighth part of their bulk. His hogs supply him with such a considerable quantity of manure, in addition to that of his farm-yard, as will enable him to maintain a constant rotation of potatoe and light corn crops. His expense in machinery is small; and the waggon that carries his flour to market brings back his coals, his groceries, and his household requisites, without additional expense. It would be necessary for him to have a considerable number of labourers, and it would be his interest to attract them around him by allowing them a small portion of land; and by being himself the purchaser of their superfluous produce, he would speedily raise a colony about him; and as the source which gave it life is connected with our subsistence, and the just necessities of our nature coeval with our wants, its prosperity must be ensured so long as we continue in the disposition to supply them. In the general complaint of the scarcity of farms, and the high rents which lands at present bear, perhaps no speculation offers stronger inducements to the young farmer, who can command a moderate capital, and who is contented to forego some of the gratifications of a large society. If he possesses activity and enterprise, and has engaged his land upon the favourable terms upon which it ought to be had at a distance from market, his success in a few years will be certain, provided the system meets with support from the public.

One of its many advantages consists in the incorruptibility of the farina; it is not like the flour of wheat, liable to decay, but it may be preserved for years sound and good, perhaps improved, but certainly uninjured by age. Thus our supply will be at all times rendered more equal, and the year of abundance will more effectually contribute to alleviate the year of scarcity: scarcities themselves are likely to be of less frequent occurrence, as we should have the advantage of two essential crops instead of one; and the weather injurious to wheat is not unfrequently favourable to the crop of potatoes, and *vice versâ.* I must also observe that the farina forms an excellent ingredient in sea biscuit, in the proportions of two parts of farina to three of wheaten flour, or even of three parts farina to four of wheaten flour. A composition of this sort is more likely to resist the effects of climate, from the incorruptible property already mentioned, than if the biscuits were wholly made of wheaten flour.

Such are some of the more leading advantages likely to be produced by the proposed introduction of the flour of potatoes into bread, and the change of system to which it would give rise. We are now, perhaps, on the eve of another alteration in the Corn Laws, calculated to render the present high prices perpetual. A Committee is sitting upon them, composed of men of landed property; and I have heard that it is in contemplation to prevent the importation of foreign wheat when the price of British wheat is under 96*s.* per quarter. This, then, is peculiarly the moment to introduce an alteration in the system, which would render such a measure unnecessary, and which, instead of perpetuating high prices, would produce amongst us all the blessings of plenty. They may be summed up in these words:—We shall have a greater regularity, and a certain reduction of price; an immense increase of consumable food; its more equal distribution through years of scarcity and years of plenty;

A consequent diminution of the poor's rates;

An increase of comforts to the poor, and to all classes of society, and a great accession to our resources in every branch of national wealth.

April 29, 1813. J. Wheatley.

EXPERIMENTS ON BREAD.

First Experiment.

No.	Weight.		Materials.	Price.	Amount.		Weight of Bread.		Cost per lb.	Observations.
	lb.	*oz.*		*d.*	*s.*	*d.*	*lb.*	*oz.*	*d.*	
1.	2	8	Flour	5¼	1	1⅛	3	6	3⅞	Good bread.
2.	2	0	Flour.	5¼	0	10½	2	11	3⅞	The same.
3.	1	8	Flour..........	5¼	0	7⅞				
	1	0	Farina	3	0	3				
	2	8			0	10⅞	3	4	3⅜	Raw potatoe flavour.

First Experiment continued.

No.	Weight.		Materials.	Price.	Amount.		Weight of Bread.		Cost per lb.	Observations.
	lb.	*oz.*		*d.*	*s.*	*d.*	*lb.*	*oz.*	*d.*	
4.	2	0	Flour..........	5¼	0	10½				
	1	0	Farina	3	0	3				
	3	0			1	1½	3	13	3½	The same.
5.	1	8	Flour..........	5¼	0	7⅞				
	2	0	Farina	3	0	6				
	2	0	Boiled Potatoes	0½	0	1				
	5	8			1	2⅞	5	7	2¾	Slight savour of the raw potatoe; crust soft.
6.	1	8	Flour..........	5¼	0	7⅞				
	1	0	Boiled Potatoes	0½	0	0½				
	2	8			0	8⅜	2	9	3¼	Well tasted bread; moist.
7.	2	0	Flour..........	5¼	0	10½				
	1	0	Boiled Potatoes	0½	0	0½				
	3	0			0	11	3	0	3⅔	The same.
8.	1	0	Flour..........	5¼	0	5¼				
	1	0	Farina	3	0	3				
	1	0	Boiled Potatoes	0½	0	0½				
	3	0			0	8¾	3	2	2¾	Excellent bread: I consider it fully as satisfying as bread made wholly of wheaten flour.
9.	1	0	Flour..........	5¼	0	5¼				
	1	0	Boiled Potatoes	0½	0	0½				
	2	0			0	5¾	1	13	3⅛	Baked heavy, but very sweet bread; moist.

Second Experiment.

No.	Weight.		Materials.	Price.	Amount.		Weight of Bread.		Cost per lb.	Observations.
1.	2	0	Flour..........	5¼	0	10½				
	1	0	Boiled Potatoes	0½	0	0½				
	3	0			0	11	2	15½	3¾	Baked heavy, but not unpleasant bread.
2.	1	8	Flour..........	5¼	0	7⅞				
	2	0	Farina	3	0	6				
	2	0	Boiled Potatoes	0½	0	1				
	5	8			1	2⅞	5	6	2¾	Baked heavy; indifferent bread.

Second Experiment continued.

No.	Weight.		Materials.	Price.	Amount.		Weight of Bread.		Cost per lb.	Observations.
	lb.	*oz.*		*d.*	*s.*	*d.*	*lb.*	*oz.*	*d.*	
3.	2	0	Flour...........	5¼	0	10½				
	2	0	Farina	3	0	6				
	2	0	Boiled Potatoes	0½	0	1				
	6	0			1	5½	6	0	2⅞	Excellent bread; improved by being kept a few days, rather over-baked.
4.	1	8	Flour...........	5¼	0	7⅞				
	1	0	Farina	3	0	3				
	1	0	Boiled Potatoes	0½	0	0½				
	3	8			0	11⅜	3	5	3½	This proportion is perhaps superior to the last, but the bread of both is excellent; improved by being kept a few days.
5.	1	0	Flour...........	5¼	0	5¼				
	1	0	Farina	3	0	3				
	1	8	Boiled Potatoes	0½	0	0¾				
	3	8			0	9	3	3	2⅞	Rather too moist, but very sweet and good, and much improved by keeping.
6.	1	0	Flour...........	5¼	0	5¼				
	1	0	Rasped Potatoes	0½	0	0½				
	2	0			0	5¾	1	12	3¼	Light pleasant bread; dark coloured.

Third Experiment.

No.	Weight.		Materials.	Price.	Amount.		Weight of Bread.		Cost per lb.	Observations.
1.	2	0	Flour...........	5¼	0	10½				
	2	0	Farina	3	0	6				
	2	0	Rasped Potatoes	0½	0	1				
	6	0			1	5½	6	2	2⅞	Heavy; discoloured; made from potatoes rasped 48 hours; flavour not pleasant.
2.	2	0	Flour...........	5¼	0	10½				
	2	0	Farina	3	0	6				
	2	0	Boiled Potatoes	0½	0	1				
	6	0			1	5½	6	1	2⅞	Heavy.
3.	1	8	Flour...........	5¼	0	7⅞				
	1	0	Farina	3	0	3				
	1	0	Rasped Potatoes	0½	0	0½				
	3	8			0	11⅜	3	9	3¼	Rather heavy; discoloured; the boiled potatoes are evidently a more desirable ingredient than the rasped.

Third Experiment continued.

No.	Weight.	Materials.	Price.	Amount.	Weight of Bread.	Cost per lb.	Observations.
	lb. oz.		*d.*	*s. d.*	*lb. oz.*	*d.*	
4	1 8	Flour..........	5¼	0 7⅞			
	1 0	Farina	4	0 3			
	1 0	Boiled Potatoes	0½	0 0½			
	3 8			0 11⅜	3 5	3¼	Good bread; toasts extremely well; indeed all bread made of a proportion of potatoe is remarkably good toasted.
5.	2 0	Flour..........	5¼	0 10½			
	1 0	Farina	3	0 3			
	1 0	Rasped Potatoes	0½	0 0½			
	4 0			1 2	4 0	3½	Rather heavy; discoloured; flavour not good.
6.	2 0	Flour..........	5¼	0 10½			
	1 0	Farina	3	0 3			
	1 0	Boiled Potatoes	0½	0 0½			
	4 0			1 2	4 0	3½	Excellent bread; superior in colour to No. 4, and rather more compact; this quantity of wheaten flour alone, as appears by No. 7, would have produced 2 lb. 10 oz. of bread; consequently the actual proportion of the produce of the potatoe in this loaf is 1 lb. 6 oz. or, as nearly as possible, one-third.
7.	2 0	Flour..........	5¼	0 10½	2 10	4	Rather dark; the appearance of this loaf indicated that the yeast used for this batch was not good.
8.	2 0	Flour..........	5¼	0 10½			
	0 8	Farina	3	0 1½			
	2 8			1 0	3 4	3¾	Excellent bread; whiter than bread made of wheaten flour alone; no flavour whatever of potatoe; not used until nine days old.
9.	2 0	Flour..........	5¼	0 10½			
	0 8	Farina	3	0 1½			
	0 8	Rasped Potatoes	0½	0 0¼			
	3 0			1 0¼	3 7	3½	Not equal to No. 10, an additional proof that rasped potatoes are not desirable.
10	2 0	Flour..........	5¼	0 10½			
	0 8	Farina	3	0 1½			
	0 8	Boiled Potatoes	0½	0 0¼			
	3 0			1 0¼	3 5	3¾	Excellent bread; rises by toasting; and was quite fresh at seven days old.

From these experiments it appears that various proportions of potatoes and farina are highly palatable in bread; but if it be calculated that not more than one-fifth part be generally introduced into consumption, the operation of that fifth is sufficiently great to produce all the effects in view, in recommending its general adoption.

Some kinds of potatoes contain a much greater proportion of arina than others, some yielding a seventh part, and even a still larger proportion of it. Experience will best determine the most desirable sort for use, as other kinds produce more abundantly in number; all these considerations should be attended to.

Potatoes may be manufactured into farina during the greater part of the year, but they lose a portion of their farinaceous matter when vegetation begins; the manufacturer, therefore, should provide an early sort to commence his operations upon in August, and cease to grind any after the months of April and May.

Frost-bitten potatoes yield nearly as good farina as the others, provided they are ground before they begin to decay; and potatoes kept over for a whole season, although of inferior value, may by this process be still rendered serviceable food for mankind. These are advantages which ought not to escape notice. Frost-bitten potatoes have hitherto become in a short time a total loss, and farmers have generally been afraid of extending their cultivation of potatoes, for fear of their being spoiled by keeping, both which inconveniences are avoided by preparing the farina from them.

Amongst the many important uses to which the farina may be applied, that of biscuit for sea store is of great consequence. Biscuits made from one part farina and two parts of wheaten flour are whiter and better than those made wholly of common flour. Baked biscuits of these proportions have been repeatedly made with uniform success; and as the farina is in itself less corruptible than wheaten flour, it is likely to produce a biscuit of a less perishable kind. Some have been already kept nearly twelve months without exhibiting any symptoms of moisture or decay; on the contrary, they were as hard and as good as on the day they were baked. Biscuit differs from leavened bread in this circumstance, that all moisture is evaporated from biscuits in baking, whilst it constitutes an essential quality in bread; the weight of bread is greater, the weight of biscuit is less, than the flour of which it is composed. It is, therefore, not inconsistent with the nature of farina that it should exhibit more moisture in bread, and yet discover no symptom of it in biscuit; and it has one convenient property, of which a baker who manufactures it may avail himself, that it may be used for bread or biscuit either in a wet or dry state; consequently the expense of drying it may in some cases be avoided. Its introduction into biscuit would produce a saving of one-third of the wheaten flour now used for that purpose, which will be found very great, when the extensive use of biscuit in the navy and army is considered. The farina of potatoes should constitute, from its general application, a part of the stores of each.

In the navy particularly, the farina would afford a great variety of food, as it forms an excellent ingredient in soup as a substitute for peas, oatmeal, and rice, and possesses the superior advantage over those substances of not being susceptible of injury. One ounce of farina will thicken sufficiently nearly one quart of water. And when this soup is flavoured with onions, celery, leeks, &c. and seasoned with pepper and salt, it forms a very palatable mixture without the aid of meat. The produce of 21 lbs. of raw potatoes made into farina, and soup formed from it, will furnish a wholesome meal to 28 persons, allowing each person a full quart, which potatoes, in their original state, would scarcely furnish a dinner to one-third of that number.

The farina of potatoes will form a thick mucilage with 26 times its weight of water.

In all cases where the farina is intended to be converted into a gelatinous state, the farina should be previously mixed with some portion of the liquid cold.

A dessert spoonful of the farina mixed with a little cold milk and salt, and added to a pint of boiling milk, and kept stirring and simmering, will form an excellent nutriment.

From various circumstances, it appears that the article called Semolina in the shops, and recommended as a nutritious diet for children and sick persons, is wholly composed of the farina of potatoes.

Certificates.

Mr. Whately, of Cork, has shown me a specimen of flour made from potatoes; and also of biscuit, made partly from this flour, and partly from the flour of wheat. I am convinced that this flour is very wholesome and very nourishing, and may be of great use to the public in supplying the deficiency of wheaten flour.

Lower Grosvenor-street,
Dec. 11, 1812.

M. Baillie, M.D.

I have examined the flour from potatoes made by Mr. John Whately, and have no doubt of its containing all the nutritious part of the potatoe, and therefore calculated to form a very good and valuable diet; and as the flour thus produced may be readily transported to any distance, and may be kept for a very long time without losing any of its nutritive properties, I think the conversion of potatoes into flour may prove highly advantageous to the community.

Soho-square, Jan. 17, 1813.

Geo. L. Tuthill.

Reference to the Engravings.

This machine is of a very simple construction, its moving part consisting of a cylinder covered with tin plates, pierced with holes, so as to leave a rough surface, in the same manner as the graters used for nutmegs, &c. but the holes in this are larger. This cylinder is situated beneath a hopper, into which the potatoes are thrown, and thence admitted into a kind of trough; where they are forced against the cylinder, which as it revolves grinds the potatoes to a pulp.

Fig. 1, Plate XXXIV., represents the machine in front, or lengthways of the cylinder; and fig. 2 is a section through the middle of it, showing also the hopper with its contents, and the manner of the action of the machine. A represents the handle, by which motion is given to the machine: it is fixed upon the end of the axis of the grating cylinder B; on the opposite extremity of this axis is a fly-wheel C, to regulate and equalize the movement. D is the hopper, into which the potatoes are put; and pressing by their weight upon the top of the cylinder as it revolves, they are in part grated away. On one side of the lower part of the hopper is an opening, closed or opened more or less at pleasure by a slider E; and the degree of opening which this has regulates the passage of the potatoes from the hopper D into the trough F: this is as wide as the length of the cylinder, and has a concave board G fitted into it, which slides backwards and forwards, by the action of levers *a a* affixed to an axis H, extended across the frame of the machine. K is a lever fixed upon the middle of this axis, and terminating in a hook at the end for the suspension of a weight L; this acts upon the board G by means of the levers K and *a a* and the rods *b*, to force or press the potatoes contained in the trough forwards against the cylinder, and complete the grating of them into a pulp.

A line *c* is tied to the end of the lever K, and passing over a pulley *d*, hangs down within reach of the person who turns the handle of the machine. By drawing this line, the weight at the end of the lever K is raised up, and by the rods *b* the board G is withdrawn to the extremity of the trough, and a fresh stock of potatoes falls out at the opening E from the hopper into the trough; then, the line being let go, the weight L presses the board against the potatoes, and forces them against the cylinder, which by its motion grates them away very rapidly, the pulp passing down the space between the edge of the lower board of the trough F, and the cylinder, which space is only a narrow crevice that nothing may pass down but the reduced pulp, which falls into a box or vessel situated between the frame at M. The board G is perforated with a number of large circular holes, to make an uneven surface, in which the potatoes remain steady, whilst exposed to the action of the cylinder upon their opposite sides.

The tin plate covering the cylinder is of course pierced from the inside outwards, and the bur or rough edge left round each forms an excellent rasping surface, for the reduction of such substances as the present. The frame of the machine requires but little explacation, merely consisting of a square frame, containing the cylinder, and supporting the hopper. This frame stands on four legs, two of which rise up to a sufficient height to carry the pivot of the axis H. The legs are morticed into four ground cells, which may be made to receive a box or chest, fitted in the manner of a drawer, to contain the pulp as it falls from the machine, or it may fall into any vessel placed underneath.

The pulp in the vessels in which it is collected should be completely immersed in water, and well stirred; the separation of the farina, by its falling to the bottom, will speedily take place. The fibrous and refuse parts should be first removed, and the farina afterwards repeatedly washed, until it no longer communicates any tinge to the water. It is then sent to the stove or drying apartment, put into boxes, and dried. Care should, however, be taken that it does not dry too rapidly. Wherever the business is conducted upon a large scale, a machine for washing the potatoes* should be erected, and it might be contrived to receive motion from the power that directs the grinding machine. The vessels containing the pulp should be so arranged that a stream of water could be made to pass through them at pleasure. The pulp, after the first separation of the farina, still retains a quantity of farinaceous matter; but it will, perhaps, be more profitable to convert it into food for hogs, with such other additions as may be thought necessary, than to steep it again for the purpose of extracting any remaining portion of farina. It will be probably best to give it to the hogs boiled, along with a moderate proportion of boiled potatoes. This disposal of the refuse pulp would nearly defray the whole expense attendant upon the manufacture of the farina.

⁂ This machine would operate with good effect for reducing apples to a pulp for making cider, as it is extremely expeditious and effectual in its operation.

ARTICLE IX.

Astronomical and Magnetical Observations at Hackney Wick.
By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North. Longitude West in Time $6''\frac{89}{100}$.

Mar. 21, Immersion of Jupiter's	12h	24′	02″	Mean Time at Hackney Wick.
2d Satellite	12	24	08·8	Ditto at Greenwich.
Mar. 25, Immersion of Jupiter's	12	24	18·6	Ditto at Hackney Wick.
1st Satellite	12	24	25·4	Ditto at Greenwich.
April 3, Emersion of Jupiter's	10	57	56	Ditto at Hackney Wick.
1st Satellite	10	58	02·2	Ditto at Greenwich.
April 8, Emersion of Jupiter's	9	24	28·4	Ditto at Hackney Wick.
2d Satellite	9	24	25·2	Ditto at Greenwich.
April 12, Emersion of Jupiter's	11	31	38·2	Ditto at Hackney Wick.
4th Satellite	11	31	45	Ditto at Greenwich.
April 15, Emersion of Jupiter's	12	00	27	Ditto at Hackney Wick.
2d Satellite	12	00	33·8	Ditto at Greenwich.

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* Models of two machines proper for washing the earth from potatoes may be seen in the Society's repository.

Magnetical Observations.

1815.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
Mar. 18	8h	40′	24°	15′	24″	1h	40′	24°	25′	16″	5h	50′	24°	14′	25″
Ditto 19	8	40	24	20	48	1	25	24	23	48	5	55	24	18	04
Ditto 20	8	30	24	14	43	1	35	24	26	13	5	55	24	16	23
Ditto 21	8	40	24	14	23	1	00	24	22	45	—	—	—	—	—
Ditto 22	8	40	24	14	53	1	40	24	24	07	5	55	24	15	23
Ditto 23	8	20	24	13	29	—	—	—	—	—	5	55	24	16	13
Ditto 24	8	15	24	12	46	1	40	24	27	33	5	55	24	17	48
Ditto 25	8	15	24	14	19	1	25	24	25	38	5	55	24	17	31
Ditto 26	8	20	24	13	45	—	—	—	—	—	—	—	—	—	—
Ditto 27	8	40	24	13	38	1	20	24	24	28	—	—	—	—	—
Ditto 28	8	40	24	14	30	1	10	24	25	08	5	50	24	15	33
Ditto 29	8	25	24	14	01	1	30	24	24	19	6	00	24	19	10

1815.

Mean of Observations in March.	Morning	at	8h	32′	..... Variation	24°	14′	49″	West.
	Noon	at	1	27	..... Ditto	24	23	40	
	Evening	at	5	54	..... Ditto	24	16	43	
Ditto in Feb.	Morning	at	8	37	..... Ditto	24	15	11	West.
	Noon	at	1	31	..... Ditto	24	21	51	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	47	..... Ditto	24	16	46	West.
	Noon	at	1	36	..... Ditto	24	20	12	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
1814. Ditto in Dec.	Morning	at	8	44	..... Ditto	24	18	02	West.
	Noon	at	1	30	..... Ditto	24	20	36	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	41	..... Ditto	24	16	20	West.
	Noon	at	1	40	..... Ditto	24	20	37	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	39	..... Ditto	24	14	08	West.
	Noon	at	1	42	..... Ditto	24	21	45	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	32	..... Ditto	24	14	33	West.
	Noon	at	1	39	..... Ditto	24	23	17	
	Evening	at	6	19	..... Ditto	24	16	50	
Ditto in Aug.	Morning	at	8	30	..... Ditto	24	14	13	West.
	Noon	at	1	39	..... Ditto	24	23	48	
	Evening	at	6	57	..... Ditto	24	16	31	
Ditto in July.	Morning	at	8	41	..... Ditto	24	13	29	West.
	Noon	at	1	42	..... Ditto	24	23	44	
	Evening	at	6	58	..... Ditto	24	17	00	
Ditto in June.	Morning	at	8	44	..... Ditto	24	13	10	West.
	Noon	at	1	39	..... Ditto	24	22	48	
	Evening	at	6	52	..... Ditto	24	16	29	
Ditto in May.	Morning	at	8	45	..... Ditto	24	13	12	West.
	Noon	at	1	44	..... Ditto	24	22	13	
	Evening	at	6	38	..... Ditto	24	16	14	
Ditto in April.	Morning	at	8	45	..... Ditto	24	12	53	West.
	Noon	at	1	48	..... Ditto	24	23	53	
	Evening	at	6	29	.... Ditto	24	15	30	
Ditto in March	Morning	at	8	52	..... Ditto	24	14	29	West.
	Noon	at	1	52	..... Ditto	24	23	08	
	Evening	at	6	11	..... Ditto	24	15	33	

1814. Mean of Observations in Feb.	Morning	at	8h	47′	Variation	24°	14′	50″	West.
	Noon	at	1	52	Ditto	24	20	58	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	52	Ditto	24	15	05	West.
	Noon	at	1	53	Ditto	24	19	03	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at	8	53	Ditto	24	17	39	West.
	Noon	at	1	51	Ditto	24	20	30	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	42	Ditto	24	17	17	West.
	Noon	at	1	54	Ditto	24	20	24	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	Ditto	24	15	41	West.
	Noon	at	1	59	Ditto	24	22	53	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	Ditto	24	15	46	West.
	Noon	at	2	02	Ditto	24	22	32	
	Evening	at	6	03	Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	Ditto	24	15	55	West.
	Noon	at	2	02	Ditto	24	23	32	
	Evening	at	7	05	Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	Ditto	24	14	32	West.
	Noon	at	1	50	Ditto	24	23	04	
	Evening	at	7	08	Ditto	24	16	43	
Ditto in June.	Morning	at	8	30	Ditto	24	12	55	West.
	Noon	at	1	33	Ditto	24	22	17	
	Evening	at	7	04	Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	Ditto	24	12	02	West.
	Noon	at	1	37	Ditto	24	20	54	
	Evening	at	6	14	Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	Ditto	24	09	18	West.
	Noon	at	0	59	Ditto	24	21	12	
	Evening	at	5	46	Ditto	24	15	25	

Magnetical Observations continued.

1815.

Month.		Morning Observ.					Noon Observ.					Evening Observ.				
		Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
April	1	8h	45′	24°	14′	17″	1h	40′	24°	22′	30″	6h	15′	24°	15′	13″
Ditto	2	8	20	24	13	28	1	25	24	25	39	6	10	24	17	24
Ditto	3	8	25	24	18	34	1	10	24	29	36	6	15	24	17	48
Ditto	4	8	20	24	13	54	1	05	24	25	38	6	20	24	15	40
Ditto	5	8	40	24	13	45	1	20	24	25	43	—	—	—	—	—
Ditto	6	8	50	24	13	10	1	10	24	25	50	—	—	—	—	—
Ditto	7	8	40	24	10	40	1	15	24	27	47	—	—	—	—	—
Ditto	8	8	20	24	12	19	1	45	24	27	19	—	—	—	—	—
Ditto	9	8	20	24	13	09	1	40	24	28	51	6	30	24	16	57
Ditto	10	—	—	—	—	—	1	40	24	28	31	6	20	24	18	12
Ditto	11	8	35	24	16	19	—	—	—	—	—	6	40	24	15	44
Ditto	12	8	15	24	18	24	—	—	—	—	—	6	45	24	18	20
Ditto	13	8	50	24	13	07	—	—	—	—	—	6	20	24	13	14
Ditto	14	8	35	24	24	57	1	00	24	31	07	—	—	—	—	—
Ditto	15	8	35	24	15	17	1	35	24	30	13	6	25	24	15	44
Ditto	16	8	35	24	19	08	1	25	24	31	14	6	35	24	17	51
Ditto	17	8	30	24	17	01	—	—	—	—	—	—	—	—	—	—

Comparison of the Monthly Variations in the Years 1813, 1814, *and* 1815.

		1813.			1814 and 1815.			Difference.	
April	Morning	24°	09′	18″	24°	12′	53″	3′	55″
	Noon	24	21	12	24	23	53	2	41
	Evening	24	15	25	24	15	30	0	05
May	Morning	24	12	02	24	13	12	1	10
	Noon	24	20	54	24	22	13	1	19
	Evening	24	13	47	24	16	14	2	27
June	Morning	24	12	35	24	13	10	0	35
	Noon	24	22	17	24	22	48	0	31
	Evening	24	16	04	24	16	29	0	25
July	Morning	24	14	32	24	13	29	1	03
	Noon	24	23	04	24	23	44	0	40
	Evening	24	16	43	24	17	00	0	17
Aug.	Morning	24	15	55	24	14	13	1	42
	Noon	24	23	32	24	23	48	0	16
	Evening	24	16	08	24	16	31	0	23
Sept.	Morning	24	15	46	24	14	33	1	13
	Noon	24	22	32	24	23	17	0	45
	Evening	24	16	04	24	16	50	0	46
Oct.	Morning	24	15	41	24	14	08	1	33
	Noon	24	22	53	24	21	45	1	08
	Evening	—	—	—	—	—	—	—	—
Nov.	Morning	24	17	17	24	16	20	0	57
	Noon	24	20	24	24	20	37	0	13
	Evening	—	—	—	—	—	—	—	—
Dec.	Morning	24	17	39	24	18	02	0	23
	Noon	24	20	30	24	20	36	0	06
	Evening	—	—	—	—	—	—	—	—
Jan.	Morning	24	15	05	24	16	46	1	41
	Noon	24	19	03	24	20	12	1	09
	Evening	—	—	—	—	—	—	—	—
Feb.	Morning	24	14	50	24	15	11	0	21
	Noon	24	20	58	24	21	51	0	53
	Evening	—	—	—	—	—	—	—	—
March	Morning	24	14	29	24	14	49	0	20
	Noon	24	23	08	24	23	40	0	32
	Evening	24	15	33	24	16	43	1	10

In deducing the mean of the observations, the morning observation of March 19, and the noon of March 24, are rejected, the variation being so great. The wind on both days was from the west, and the weather cloudy.

March 31.—Immersion uncertain to 10″.

Rain fallen	Between noon of the 1st Mar. Between noon of the 1st Apr.	1·528 inch.

Comparison of the Yearly Variation.

	First 12 months observ.			Second 12 months observ.		
	Morning.	Noon.	Evening.	Morning.	Noon.	Evening.
April	24° 09′ 18″	24° 21′ 12″	24° 15′ 25″	24° 12′ 53″	24° 23′ 53″	24° 15′ 30″
May	24 12 02	24 20 54	24 13 47	24 12 49	24 22 13	24 16 14
June	24 12 35	24 22 17	24 16 04	24 13 10	24 22 48	24 16 29
July	24 14 32	24 23 04	24 16 43	24 13 29	24 23 44	24 17 00
August	23 15 55	24 23 32	24 16 08	24 14 30	24 23 48	24 16 31
September	24 15 46	24 22 32	24 16 04	24 14 33	24 23 17	24 16 50
October.........	24 15 41	24 22 53	— — —	24 14 08	24 21 45	— — —
November......	24 17 17	24 20 54	— — —	24 16 20	24 20 37	— — —
December	24 17 39	24 20 30	— — —	24 18 02	24 20 36	— — —
January........	24 15 05	24 19 03	— — —	24 16 46	24 20 12	— — —
February	24 14 50	24 20 58	— — —	24 15 11	24 21 51	— — —
March...........	24 14 29	24 23 08	24 15 33	24 14 49	24 23 40	24 16 43
Mean	24 14 36	24 21 42	24 15 41	24 14 42	24 22 22	24 16 28
				24 14 36	24 21 42	24 15 41
Difference......	....			+ 6	+ 40	+ 47

Mean of the three differences........ + 31″

◆

(To Dr. Thomson.)

MY DEAR SIR, *Hackney Wick, April* 17, 1815.

I HAVE the pleasure to send you the conclusion of the second year's observations on the variation of the magnetic needle. The number of observations made in the first year, and set down in the *Annals of Philosophy*, were, in the morning 294, at noon 265, and in the evening 141. But it is to be remarked that the numbers set down are the means of the observations made with two needles, and each consisting of 14 readings off on the arc of the instrument. If, therefore, the former numbers be multiplied by 28, the total amount will be 19600, and the number in the second twelve months will be found 22764. As every attention was paid in making the observations, and the two needles rarely differed three minutes, seldom two, it may be presumed that the variation is truly determined, and has not yet arrived at its maximum, the annual increase being 31″.

It certainly would be a desirable circumstance if observations were made, at this period, in different parts of the world, especially in those places where the variation is great; and as I perceive in Number xxv. of the *Annals of Philosophy*, that Mr. Scoresby, jun. of Whitby, has kept meteorological journals during his voyages to Davis's Straits in the years 1813 and 1814, perhaps he, or some other Gentleman, may favour your readers with observations on the variation of the compass in that part of the world. That a comparison may be made of the present variation with that in the year 1790, I have inserted Capt. Brown's observations, on whose accuracy dependance may be placed. His journals are in my possession. He commanded the ship Butterworth; and was furnished with an accurate azimuth compass, made by Mr. Dolland for the purpose of making the observations now inserted.

Captain Brown's Observations.

Variation.	Lat. Obs.	Longitude.	
79° 42′ W	72° 46′ N	—° —′	By Sun and Moon.
79 00 W	72 05 N	— —	
78 15 W	71 26 N	— —	
73 44 W	— —	— —	
74 00 W	70 58 N	54 14 W	
73 40 W	70 55 N	— —	
72 00 W	70 05 N	— —	
71 00 W	66 59 N	57 04 W	
70 40 W	65 44 N	59 31 W	
70 00 W	63 40 N	59 22 W	
68 00 W	63 34 N	58 33 W	

I am not acquainted with the alteration in the variation which has taken place on the Continent. From several observations I made in the year 1787, at La Rochette, near Neuchatel, in Switzerland, the variation was 19° 7′ 14″ W. The latitude of this place, from many observations, is 47° 00′ 15″ N.; and the longitude, deduced during my residence there from solar eclipses and occultations of the stars by the moon, 28′ 40″ E. in time, or 7° 10′ 00″ in space, of Greenwich.

Not being aware that any observations have been made to determine the situation of the supposed highest mountain in Europe, I take the liberty of sending you the latitude of Monte Blanc, from an observation I made under very favourable circumstances, on its summit, in the month of August, 1787; the latitude, deduced from the meridional altitude of the sun, is 45° 49′ 59″ N.; and as the summit bears from Neuchatel by the compass S. 20° 54′ 07″ W. by using the difference of latitude and the true bearing, the longitude in space is 3′ 10″ W. of Neuchatel, and consequently 7° 6′ 50″ E. from Greenwich. The lake of Neuchatel I found to be 1428 feet above the English Channel.

I remain, my dear Sir, yours very sincerely,

MARK BEAUFOY.

Article X.

Observations on the Uses of the Dorsal Vessel, or on the Influence which the Heart exercises in the Organization of articulated Animals, and on the Changes which that Organization experiences when the Heart or the Organ of Circulation ceases to exist. By M. Marcel de Serres.

(*Continued from* p. 199.)

This last series of researches being finished, it remained to determine the cause of the contractions and dilatations of the dorsal vessel, and of what importance that vessel was to the general economy of insects. These contractions and dilatations might depend on various causes, or only upon the peculiar structure of that organ; though it was difficult to adopt such an opinion, because the absence of vascular vessels announced that the humour contained in it did not circulate. To determine the point I began by examining what influence the muscles which surround the dorsal vessel might have upon its contractions.

The mole cricket, being a very lively insect, was chiefly employed by me in these researches. The dorsal vessel being brought in view, I removed the abdominal muscles (and the adipose membranes of necessity) in the middle part of the abdomen, while I left the dorsal muscles quite entire in the upper and lower part of the abdomen. This being done, I remarked that the dorsal vessel always contracted in those parts where the muscles had not been removed, and these contractions were the less lively the nearer they came to the muscles that had been removed. However, by degrees, some contractions took place in the parts of the dorsal vessel from which the muscles had been removed; but they were always weak, and seemed to proceed from portions in which the muscles still existed.

In other cases I removed entirely the dorsal muscles; then the contractions of the dorsal vessel became weaker by degrees, and ceased at last, though after a very considerable interval. Dr. Encontre* was so good, at my request, as to repeat these experiments. He observed that, after removing the dorsal muscles, the contractions of the dorsal vessel diminished by degrees in the rings from which these muscles had been removed; while they continued still strong in those rings where the muscles still remained. In a ring in which he had left only some traces of muscle, the dorsal vessel still continued to contract, when its contractions had ceased in all the parts from which the muscles had been removed. I must remark, that the insects subjected to these experiments are not dead, when all the dorsal vessels have been removed; but what is still more singular, they continue to live after the dorsal vessel has been entirely removed. I have seen the caterpillar of a sphinx atropos breathe for six hours after I had removed the dorsal vessel. The inspirations and expirations still continued, and air bubbles broke from the ends of the tracheæ which had been cut at the removal of the dorsal vessel. If this organ were a heart, how could insects possibly live so long after being deprived of it. To be convinced of the difference, let the heart be removed from those animals in which it exists even in the least complicated state. Not one will resist its loss, most of them will be dead before the removal can be completed. I must observe, that after the removal of the dorsal vessel, insects are no longer able to move about; but life notwithstanding still continues.

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* He is the son of M. Daniel Encontre, known with eclat as a mathematician.

This intensity of life is found in all animals that have no centre of action: other animals die very speedily. One would think there ought not to be a great difference in this respect between the *faucheurs* and spiders, or between the scolopendras and scorpions, and yet there is a very great one. The scorpions and spiders die almost immediately after exposing their heart; while insects live often six or seven hours after removing the organs most essential to life. Yet both have a nervous system composed of a series of ganglions; but the insects have no centre of circulation, or, which is the same thing, they have no heart.

We have seen the influence of the dorsal muscles on the contractions of the dorsal vessel. As this influence might not be the only one, I endeavoured to determine those which might belong to the tracheæ and nerves. Before engaging in this new set of experiments, I wished to determine whether by means of metallic exciters I could render the contractions of the dorsal vessel more frequent and stronger. When the two exciters were brought into contact the insect exhibited symptoms of pain; but the dorsal vessel, instead of increasing in liveliness, beat always more and more slowly. On continuing the contact of the wires of zinc and copper between the membranes of the dorsal vessel, the humour contained in this vessel gradually coagulated, and the contractions ceased by degrees. Thus the galvanic agent in the present case produces effects similar to the chemical, with this difference, that it acts less rapidly.

It might be concluded, *à priori*, that the tracheæ have a considerable influence on the contractions of the dorsal vessel, on account of the great number of them which enter it and constitute one of its coats, and on account of the great influence of air on the organs of insects. But great difficulties prevent us from determining that influence; the most insurmountable of which is the impossibility of separating the tracheæ from the dorsal vessel: for, without such a separation, we cannot judge of the influence which they exert on its contractions. I have not been able to surmount these difficulties in those insects which have tributary tracheæ. So that I can only depend upon one experiment, which succeeded very well on a caleoptera lamellicornis. I removed with all possible care, in the *ateuchus semi-punctatus*, the vesicular tracheæ which surround the dorsal vessel, as well as their ramifications. When they were removed the contractions of the vessel diminished by degrees. I had indeed removed some muscular fibres; but in so small a quantity, that I do not believe that cause had a remarkable influence. These contractions have continually gone on diminishing, and the diminution was much greater than what is the consequence of pain, and of the cessation of certain functions. I believe, therefore, that the contractions of the dorsal vessel are proportional to the quantity of adipose tissue; the energy of the muscular fibres that surround it, and of the number of tracheæ which enter into it, or of the air which it receives.

Let us see now whether the nerves have not some influence on these contractions, as the dorsal vessel receives a certain number of them. To determine this influence, I removed the spinal marrow with the nerves coming from it, as far as I could distinguish them, and then examining the contractions of the dorsal vessel, I could not perceive that they were sensibly enfeebled. I repeated this experiment on a great number of individuals of different orders, and I always obtained the same result. Hence I conclude, that the nerves have no very sensible influence on the contractions of the dorsal vessel. It is possible, that in all these animals in which the nervous system is very much divided, and the principal centre has little preponderance, the nervous influence is less distinct. This I shall endeavour to determine by future researches.

It remained still to determine whether the contraction of the dorsal vessel does not depend in part on the circulation of the liquid which it contains. We have already observed, and the observation, since the time of Malpighi has been renewed by all anatomists, that the humour of the dorsal vessel has a very irregular motion, and that the fluid often appears to go from the head towards the tail, and at other times to take a contrary direction. Sometimes the dorsal vessel contracts at its two extremities, while these contractions do not take place in the middle of the organ, especially if the dorsal muscles have been removed in this part. So that the liquid in the dorsal vessel appears at one time to move with extreme rapidity, at another very slowly, without any apparent connexion between this irregularity and the state of the insect. But how can this irregular motion be reconciled with the circulation of a fluid analogous to the blood, and how can a circulation take place without vessels? All these facts are in themselves so conclusive, that it is difficult to consider the dorsal vessel as a heart, and, of course, to admit that the contractions are produced by a liquid in circulation. In the second place, if we pierce the heart of any animal whatever, the moment the blood arrives it rushes out in a quantity proportional to that which enters the heart. It was important, therefore, to determine whether this would happen when the dorsal vessel of insects is punctured. For that purpose I chose several very lively individuals. I pierced the dorsal vessel in different parts at the time when the liquid had flown back, but I never perceived the least moisture issue out of it. As it is difficult to determine this point in a satisfactory manner, I repeated the experiments with all the precaution possible. The results were constantly the same: no liquid flowed out. If this were the only proof that could be given of the non-circulation of the humour contained in the dorsal vessel, it would not be of much importance; but, joined to those that we have already made known, it acquires a certain force, and even a considerable weight. But it may be said, that the reason why the humour does not exude when the dorsal vessel is punctured, is because it is too thick. I am very much inclined, I own, to that opinion; for this humour has always appeared to me very little liquid, and in the voraceous larvæ it has a remarkable consistence. This consistence, joined to some other particularities, has put us on the way to determine with some precision the functions of the dorsal vessel in those animals that have no other circulation than that of air. The different movements of contraction and dilatation which we remark in the dorsal vessel, can never induce us to consider that organ as a heart; since, in the animals like the Naiades (*Nerèis*, Lin.) in which there exists only a single organ, that of digestion, we observe pulsations as distinct as those exhibited by the heart of other animals. Therefore the pulsations of the dorsal vessel ought not to make us decide in any manner on the use of that organ, nor lead us to consider it as a heart.

In all animals which have a system of circulation and respiration, the influence of the one of these on the other has been perceived.* Thus often when the respiration is entire, the circulation is only half so, or when the circulation is complete, the respiration does not operate in a complete manner; so that a demicirculation, multiplied by a complete respiration, or *vice versâ*, gives always equal products; that is to say, a semioxygenation of the blood. But in mammiferous animals, in which the circulation and respiration are complete, the oxygenation of the blood is equally so; and in birds which have a complete circulation with a double respiration, the oxygenation of the blood is doubled, in consequence of the quantity of air that combines continually with it.†

If the dorsal vessel of insects is an organ of circulation, it ought to experience the influence of the organs of respiration like the heart of animals with vertebræ. This influence ought to be so much the greater, as insects, like birds, have a double respiration. The air penetrates into all the parts of their bodies, as it does in birds. It continually bathes their nutritive humour, or their blood, which has this particularity, that it is not confined in vessels; just as in birds it acts upon the blood in the great circulation, at the same time that the small circulation experiences the action of the air in the lungs. Some insects have even peculiar reservoirs of air in their bodies, reservoirs so numerous, that those which are provided with them ought to have a respiration more than double. These reservoirs of air, called *vesicular tracheæ*; but which may likewise be called *pneumatic pockets*, exist only in those insects which have to exert a great muscular force, and which, having great spaces to traverse, ought to have their body specifically light. From what has been said, it is evident that the air ought to have a great influence on the general economy of insects. We shall see hereafter, that it is the only fluid which has a real circulation in them. Now the greater this influence is, the more ought it to exert itself on the organ of circulation, provided such an organ exist. As the dorsal vessel has been considered by different anatomists as coming in place of the heart in insects, let us see whether the influence of the respiratory organ is sensible on this vessel. But to lay open that influence in a more certain manner, let us examine in the first place the organs of respiration themselves, and see whether, when these organs undergo modifications, the dorsal vessel feels their effects.

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* See Cuvier's Anatomie Comparée, t. iv. p. 167.

† These speculations about the oxygenation of the blood are quite vague and uncertain. T.

All the organs of respiration in insects may be reduced to simple tracheæ. These organs indeed have not all the same composition; we must therefore divide them into two orders: the first, which we shall call *tubular tracheæ*, on account of their disposition, are composed of three membranes, one external, one internal, and one intermediate. The two first are formed of a cellular membrane, pretty thick, and very extensible; while the intermediate one consists of a cartilaginous string, rolled up into a spiral, which may be unrolled with great facility. It is the convolutions of this elastic string which forms those brilliant and silvery looking conduits, destined to hold air, and to transmit it to all parts of the body. These tracheæ are always kept stretched by means of this cartilaginous string. They form tubes, and have a great elasticity, which enables them to dilate, when the air, continually circulating in them, expands. These tracheæ are likewise the only ones which are ramified, the multiplied branches of which go to all parts of the body, to communicate the impression of air, the aliment of life as well as of flame.

The second order of tracheæ, or the *vesicular tracheæ*, do not form tubes like the preceding; they present, on the contrary, pouches more or less extended, which communicate with one another by means of ramifications, always single, and never branched like those proceeding from the spiral tracheæ. These vesicular tracheæ are composed of two very white cellular membranes, very supple and very extensible. As these tracheæ have not the spiral string observable in the first order, they never communicate immediately with the air, but always by means of spiral tracheæ. Accordingly, in the species which have need of a great quantity of air, and which have very extensive vesicular tracheæ, we observe a particular apparatus destined to supply the elasticity which they want.

This apparatus is composed of hemispherical cartilaginous hoops, furnished with particular muscles, and which, in consequence of this disposition, may be considered as a kind of ribs. In fact, these ribs elevate at every inspiration the vesicular tracheæ and increase their size, allowing them to receive a greater quantity of air; on expiration they depress the tracheæ, and thus serve to drive out the air. These ribs, fixed by their base to the coriaceous envelope, are only moveable by their upper parts. They exist only in those insects that have vesicular tracheæ of a certain extent. We do not observe them in the lepidopteræ, the caleopteræ lamellicornes, and the dipteres, in which the vesicular tracheæ are scarcely a half millimetre (0·0196 inch) in extent. In certain orthopteres, on the contrary, as the *gryllus*, *truxalis*, *acrydium*, in which the tracheæ are large, several millimetres in extent, the cartilaginous hoops, or ribs, always exist, and are in these animals very necessary.

Such are the organs which serve in insects as reservoirs of air. Very different from the lungs and bronchiæ, they are not placed in any particular part; we see them, on the contrary, spread every where, with a sort of profusion: so that there is no part of these animals that does not breathe and receive the action of air.

The general disposition of the tracheæ, and the different particularities of the organization of insects, have occasioned the manner in which these organs communicate with the external air, a mode quite different from what we observe in other animals. The organ of respiration in insects being very much extended, and very much ramified, a single opening would not have been sufficient to distribute the air in all parts with that regularity and abundance that the circulation of that fluid required. Accordingly, the openings by which the tracheæ receive air are always more than one, having never fewer than two. Most commonly that number varies from eight to twelve, and sometimes amounts to 20. These openings have been in general called *stigmata*. But, as in certain species there exist some which open and shut, by means of an apparatus of which the true stigmata are destitute, we shall divide them into two different orders. We shall call the first *simple stigmata*, and the second *tremaer* * *stigmata*.

The simple stigmata are most frequently placed on the sides of the body between the folds of the membrane of the back and abdomen. They are always disposed in pairs, presenting, in general, a round opening like a button-hole, with a cartilaginous border. Sometimes, however, this border is totally wanting, and the stigmata are then surrounded by a cartilaginous scale of a different colour, from the coriacous envelope of the body. In the caterpillars the stigmata are equally formed of small cavities, pretty deep, the edges of which are surrounded by a brown mark, and at the bottom of which we discover a stripe of the same colour.

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* The word is derived from τρημα, *opening*; and αηρ, *air*.

A good deal might be said about the number and situation of these stigmata, if we considered these organs in the different orders of insects. But as these details would carry us too far, we shall satisfy ourselves with observing, that the number of these stigmata is so much the more considerable the more need the animal has of air. Accordingly, in caterpillars we reckon 18 or 20 stigmata, and in a great number of the orthopteres there are 12 or 16, without reckoning the two tremaers. In certain species, which only require a small quantity of air, the number of stigmata does not exceed two; among the apteres the *faucheurs* exhibit this disposition. Besides these stigmata, we see in several orthopteres two openings situated at the bottom of the abdomen; but as their use is not the same as that of the stigmata, we think it unnecessary to describe them here. But we shall not pass over the large oval opening which exists in the breast of the locust above the first pair of paws. It communicates with a large trachea, which extends the whole length of that paw. This opening serves too evidently to introduce air into the superior part of the body, not to be considered as a true stigma; especially as it is formed like the other stigmata of the extremities of the tracheæ. As to the situation of the stigmata, it is subject to numerous variations, always depending on the quantity of air which the insect requires. The more air is required the more is the situation of the stigmata such as to admit an easy introduction of it. The orthopteres, most of the larvæ, the lepidopteres, the hymenopteres, and the dipteres, seem the most favoured in this respect. In the caleopteres the tracheæ are generally placed not advantageously for the introduction of air. Some of those that live in water are obliged when they wish to respire to elevate a little their *elytres*, that the air may enter more easily into their tracheæ.

The second kind of stigma, which we shall call *tremaer*, is composed of an oval opening, which opens and shuts by means of two moveable horny pieces, set in motion by a peculiar apparatus of muscles. The general form of that opening is that of an elongated oval, the greatest diameter of which is from below upwards. In expiration the moveable pieces separate from one another; they shut on the contrary when the inspiration is terminated. These moveable pieces open from within outwards, and do not separate from each other more than half a millimetre, (0·0196 inch.) The movements of the tremaers agree sufficiently with those of the stigmata, and like them they communicate with the tracheæ. It is even very easy in dissecting these parts to recognize the tracheæ which go to them, and the muscles which move the tremaers, two in number, or one for each tremaer. These muscles are destined to separate or open the moveable pieces of the tremaers: accordingly, they shut when these muscles cease to act. These muscles are composed of fibres sufficiently distinct, which are attached in the second cavity of the thorax in the erismes. Hitherto we have observed the tremaers only in a certain number of orthopteres, where they present very various forms. In general, however, they exhibit the arrangement of which we have spoken; and it is only in the mantes that we see them situated on the lateral and external side of the thorax, between the superior and inferior portion of that part. The tremaers have then a triangular form. Instead of presenting two moveable pieces, they have only a single piece moved by a particular muscle. It is always by means of this muscle that the moveable part is raised up; and as soon as the muscle ceases to act, the moveable part falls down and shuts. The membrane situated below the tremaer is so thin, that we can see in the expirations and inspirations the elevation and depression of the trachea, in proportion as the air enters or goes out.

Though the position of the tremaers experiences some variations, being sometimes situated in the neck, sometimes in the breast, this is not the case with the number of these parts. We never see more than two, the size of which is always proportional to the quantity of air which the insects respire. Besides that manner of receiving air the insects swallow it likewise by the mouth. What is obtained in that way indeed can only make its way to the organs of nutrition; and as we have elsewhere explained its influence on digestion,* I shall not resume the subject at present. The last mode of respiration which certain insects present, is to have their stigmata placed at the anus. Those exhibiting that organization are chiefly the insects that decompose water, as the larvæ of libellulas and dytics. These stigmata, or the openings which allow the water to escape, are surrounded by small triangular and moveable pieces, the principal use of which is probably to keep away those bodies that might hinder the introduction of water into these stigmata, and at the same time to shut the opening exactly when the insects suspend this introduction. Accordingly, when these insects choose to introduce water into their respiratory organs, they separate the moveable pieces of which we have spoken, and they shut them in the contrary case. But when these pieces are separated we easily distinguish the round opening by which water is introduced into the respiratory apparatus. This opening presents a diameter of about a millimetre, 0·03937 inch,) and it is easy by means of it to let out the water which may exist in the interior of the body.

Considered with respect to their respiratory organs, insects form three distinct classes; namely, those that breathe air immediately; those that live in water and are obliged to come to the surface of that liquid in order to receive the impression of the air, though they likewise lay hold of that contained in the water; and those that decompose water in order to obtain its oxygen. It is obvious that those insects which breathe air immediately, ought to be the only ones in which we find the two kinds of tracheæ of which we have spoken. The vesicular tracheæ would not have sufficient elasticity to drive out the superabundant water, which the insect introduces into its tracheæ. Perhaps this elasticity would not even be sufficient for those which live habitually in water, but do not decompose that liquid. In the second place, the insects which decompose water ought to be the only ones that have but one stigma. This stigma ought to be so placed that the animal may receive the water necessary for it; and we see that whenever this disposition exists, it is always at the anus that this opening is found. But before passing to the description of all these different kinds of organization, we shall here give a table which will render them obvious at one view.

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* See my Memoir on the Intestinal Tube of Insects, inserted in the Annales du Muséum d'Histoire Naturelle, tom. 19.

I. *Respiration in Air.*

I. With tubular tracheæ	Division I.—Arterial tracheæ.	
	Division II.—Pulmonary and arterial tracheæ.	
II. With vesicular tracheæ......	Always two orders of tracheæ	1. With cartilaginous hoops, or a kind of ribs. 2. Without cartilaginous hoops or ribs.

II. *Respiration in Water.*

Only with tubular tracheæ ...	Division I.	Breathing by true stigmata, and coming to the surface of the water to breathe air.
	Division II.	Breathing by an opening placed at the anus, decomposing water.

This table shows us that the result of respiration, or the oxygenation of the blood, or of the humour which comes in place of it in insects,* ought not to be the same in the different modes of organization which animals present. We may very well suppose that the insects which decompose water in order to take possession of its oxygen, have only a demi-respiration, while those that receive air immediately, have a complete respiration. Those which receive air immediately, and have very considerable vesicular tracheæ, with an apparatus intended to keep them always full of air, ought to have a respiration still more complete. And to adopt the ratio which we have already pointed out, though far from exact, the respiration may be very well doubled by that complication of means. At least it is certain that the cartilaginous hoops which we have compared to the ribs of animals with vertebræ, do not exist along with vesiculary tracheæ, except in those species which have occasion for a great muscular power, from the great distances which they are destined to travel. Such, for example, are the locusts (*criquets*), as famous for their emigrations, as for the ravages which they commit in those countries where they stop in order to feed. Finally, the ordinary vesiculary tracheæ, or those which, not being very large, do not require a particular apparatus for moving them, are found only in species which require great muscular exertion, or which, flying a great deal, ought to be able to diminish their specific gravity. The caleopteres lamellicornes, and the lepidopteres and dipteres, are the insects in which this arrangement is most evident. Before passing to the particular description of the respiratory organs of insects, we ought to explain what we mean by arterial tracheæ and pulmonary tracheæ. Insects exhibit in general two orders of tracheæ, the uses of which not being the same (though their organization is not very different) deserve to be distinguished.* The one goes directly to the stigmata, takes air immediately, and distributes it to the different parts of the body. The other does not receive air immediately. It only communicates with the external air by means of the first order, and generally, being larger than the arterial tracheæ, serves as a reservoir of air. Their direction, in general, is more regular, and their ramifications much less numerous. These two orders of tracheæ having then a different object to fulfil, I thought that in order to render the description of these vessels the clearer, it would be advantageous to distinguish them. I have called the one arterial tracheæ, because they are branched like these vessels, and carry air to all parts of the body, as the arteries distribute blood in those animals that have a real circulation. But to perceive how far this analogy is well founded, we must recollect, that in insects air is the only fluid that is really circulated. As to the pulmonary tracheæ, as they serve as reservoirs of air to enable it to act on the different parts of the body, this name was the most suitable that offered itself. The two orders of tracheæ in insects were recognized by Swammerdam, and, in general, he calls those tracheæ arteries, which we have distinguished by the name of pulmonary tracheæ. The arterial tracheæ of Lyonnet are the same as ours, and, in fact, no others exist in caterpillars. It may be proper to observe, that the two orders of tracheæ do not always exist; but the arterial are never wanting. Perhaps in the species in which we see only arterial tracheæ, the parts require a speedy impression from the air.

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* By the expression *oxygenation of the blood*, I do not pretend to affirm that in respiration a portion of oxygen is fixed in the blood, and that there is always a diminution in the volume of oxygen. Though this opinion has been supported by the most skilful chemists, as Lavoisier, Goodwin, Davy, and Berthollet, we must acknowledge that the experiments of Crawford, and especially those of Allan and Pepys, seem to oppose that opinion. Thomson, to whom we likewise owe experiments on the same subject, has observed, that the diminution of volume of the oxygen is not constant; and he considers it as an effect unconnected with respiration. But whether respiration produces only a decarbonization of the blood, that is to say, that the volume of oxygen absorbed represents exactly the volume of carbonic acid gas expired; or whether there be a diminution in the oxygen, besides that which has served to the production of carbonic acid at the expense of the blood, we thought that we might employ the word oxygenation of the blood, because it is more convenient in explaining the physiological phenomena of respiration.

We shall put the reader in mind here, that M. Vauquelin proved long ago the necessity of oxygen in the respiration of insects, and that common air retains scarcely any oxygen when these animals can no longer live in it. The air which we expire, on the contrary, is composed of three carbonic acid, 18 oxygen, and 79 azote. See Ann. de Chim. tom. xii. p. 273 and 282.

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* The greater number of anatomists had observed long ago, that there exist two orders of tracheæ in insects, the one destined to introduce air into the body, the other to carry it to all parts of the body. Reaumur thought that the insects drew in air by their stigmata, but that they threw it out from all parts of the body. Mem. tom. i. p. 399, 409.

(*To be continued.*)

ARTICLE XI.

ANALYSES OF BOOKS.

I. *Researches into the Physical History of Man.* By James Cowles Pritchard, M.D. F.L.S. of Trinity College, Oxford; Fellow of the Wernerian Natural History Society of Edinburgh, and of the Medical Society of London; and extraordinary Member of the Royal Medical Society of Edinburgh. London, J. and A. Arch, 1813.

The physical history of man presents a field not less interesting than unexplored, yet Dr. Pritchard is the first who has exhibited it in a connected shape. By assembling together the most important facts he has been enabled to deduce general conclusions of considerable moment, some of which are so very remarkable as to excite something more than common surprise. The naturalists of all times have overlooked, whimsically enough, the history of our own species, and have devoted the whole of their attention to inferior animals. Yet in the course of their researches they have developed certain general principles, which may be applied to all parts of animated nature. The particular application of these principles to the human race appears to have been first made by Dr. Pritchard, although the subject has occasionally and casually engaged the attention of *speculative* philosophers, from the days of Aristotle to those of M. de Buffon. As it might naturally be expected, a thousand crude conjectures have supplied the place of accurate observations and reasonings. It it needless to repeat them here; it may suffice that, in general, the ancients considered the human race to be of one species, and ascribed every variation in point of form and colour to the effects of climate, the particular examples of which are abundantly ludicrous. The same opinion, variously modified, has been maintained by many moderns; but with most remarkable eloquence by Count de Buffon, in his Histoire Naturelle. This last named naturalist, however, did not possess a store of facts to resolve the difficulties that press on his peculiar modification of this hypothesis. Lord Kames stands at the head of another set of philosophers who maintain that mankind have sprung from various stocks, and that each particular race is especially adapted by Providence to the region in which they exist.

Dr. Prichard, without pledging himself to any opinion, investigates in the first place the criteria by which species are to be distinguished, and after examining those already offered, particularly that of John Hunter, who considered the sterility or fruitfulness of a hybrid the proof of difference or identity of species of the parents, he endeavours to lay down a general rule, derived from analogy, which, although imperfect, yields more satisfaction to the inquirer, than any solitary assumption can do. It has, too, the sanction of two of the most eminent naturalists of the present day. The rule which Dr. Pritchard lays down is, that after having observed a number of the changes produced in living bodies by adventitious causes, when any particular deviation is found frequently to recur, all parallel diversities may be ascribed to analogous causes, although the relations between these latter causes and their effects should not be so distinctly traced as in other more ordinary events. According to this rule, all the remarkable varieties of mankind, when compared with those of the inferior animals, particularly when domestication has displayed its effects, fall within the limits of one species; and Dr. P. makes the inference, that as the strictest analogy exists between the changes which almost the whole of the inferior tribes have a tendency to assume under known circumstances, and those diversities existing among men, it is consistent with the strictest rules of philosophizing to infer, that these changes originate in "the principle of natural deviation, and furnishing no specific distinction."

The next branch of the inquiry is how far men are to be considered as having *all* proceeded from *one* common stock. This question has been treated of by several writers purely on historical evidence; Dr. Pritchard, however, without disregarding the powerful arguments afforded by the historical researches of Sir W. Jones, Bailey, and others, still proceeds on analogy, and endeavours to solve the general problem as it regards all organized tribes, but more particularly animals. With much acuteness he has examined the distribution of animals, more especially in those vast regions of the southern hemisphere, so little known until the important discoveries of Cook, Bougainville, Wallis, Flinders, and others, had removed the veil which was spread over Nature; and concludes by inferring, "that every existing species may be traced with probability to a certain point which appears to have been its original abode; and that few or no species have been found in countries separated from their primary seats by barriers which their locomotive powers and peculiar structure do not enable them to surmount." This inference, if true in general, includes the particular case of the human species.

Having thus established the criteria of species, and rendered it probable that species were originally confined to one point, it is necessary to determine as far as possible, what are the causes that produce the very extraordinary diversities which exist among mankind. The commonly maintained hypotheses of those who adopt the opinion of the unity of species among men, that a change produced in the white European by heat and food becomes hereditary, is exceedingly inconsistent with notorious facts. Nor does it receive any additional weight from its antiquity; that only affords an additional proof of the facility with which even absurdity may be propagated under the sanction of illustrious names. Dr. Pritchard shows that the previous opinions are irreconcileable with fact, and supposes that certain causes exist, which, acting on the parents, "influence them to produce an offspring endowed with certain peculiar characters, which characters, according to the law of Nature, become hereditary, and thus modify the race." In order to develope those causes, Dr. P. shows that, although climates produce very remarkable variations in individuals, both in the animal and vegetable kingdoms, yet the only permanent effects seem to be derived from cultivation and domestication. Of these effects there are ample and apposite proofs in our cultivated fruits, and among domestic animals. The original stocks remain unaltered, while the most extraordinary changes are brought about by culture. Dr. P. institutes a parallel between the culture of plants, the domestication of animals, and the civilization of man; and, while he allows that some connate varieties *may* be produced by climate, he insists that the condition of man in social life influences more extensively the physical structure than any variety of latitude or local temperature: and concludes by ascribing those remarkable diversities among mankind, not to any moral cause, but to physical causes connected with a particular mode of life. Of this, several interesting illustrations might be cited, in addition to those given by Dr. Pritchard.

Having established the probability of these opinions, he proceeds to determine whether the original race were white or black; and having shewn that black, or at least a very dark brown, in all the inferior tribes, is the primitive colour; and that the same complexion prevails among all savages, and a gradual change towards white occurs through successive races of semi-barbarians to perfect civilization, he infers that the prototype of the human race was a Negro. However satisfactorily this conclusion may be drawn, it is necessary to afford some proof, and accordingly the author adduces numerous illustrations calculated to give considerable force to his argument. He urges with much force the singular varieties in form and complexion among the widely scattered nations of the Pacific Ocean; who exhibit almost every variety, from the savage Papuan to the highly refined European. He proves, by similarity of language, of religious rites, and other points of coincidence, their common origin; and then applies the results obviously obtained from these individuals to the whole of the human race.

The outline then of these Researches into the physical history of man, is, that as uniform diversities are produced by certain known causes; so, all similar or analogous diversities should be ascribed to analogous causes. That as an analogy does actually exist between certain phenomena among mankind, and those which commonly occur among the tribes, they must be all ascribed to one uniform principle of Nature. That every individual species may be traced to *one* original birth-place, and consequently, that the human species have had an original abode. That the cause which produces the diversities among mankind is civilization, which does not produce any effect on the parent which is hereditary; but, by acting on the parent physically, influences the production of certain offspring. And finally, that the change in form and complexion has been from black to white; and that the primitive race of men were negroes. After these conclusions, the remainder of the volume is chiefly devoted to historical researches in corroboration of the last startling inference. From these inquiries it appears, that the most ancient nations of whom we have any record were negroes, that they have gradually lost their characters, and that many, or most of the now existing nations, though widely differing in form and complexion, may be traced to them.

The Hindoos and Egyptians are certainly the oldest nations of antiquity. The correspondence between them is very remarkable, and the importance of the resemblance increases as we find it increase in proportion to the antiquity of the period to which we refer. In morals, in politics, and endless superstition, the resemblance is so unbounded, as to leave no doubt of an identity of origin. The pretensions of the Babylonians, who alone rival them in antiquity, will not admit of scrutiny. It will be worth while to ascertain the physical and mutual connexion of these early races, as by doing so, some light would probably be thrown on the history of the species in general. These nations appear originally to have been absolute negroes.

It is to be hoped that Dr. Pritchard will pursue this very important subject through all the forms of which it is susceptible, and to which he appears so fully competent to give interest.

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II. *Traité des Poisons tirées des Regnes Mineral, Vegetal, et Animal, &c. A Treatise on the Poisons of the Mineral, Vegetable, and Animal Kingdoms, or a general Toxology considered as related to Physiology, Pathology, and Medical Jurisprudence.* By M. P. Orfila, Pensionary Naturalist of Spain, Doctor in Medicine of the Faculty of Paris, Professor of Chemistry and of Natural Philosophy. Vol. I. Paris, 1814.

This work, if we are to form a judgment from the part of it which is already published, promises to be very complete. The author is a practical chemist, as well as a physician, and he has laid it down as a rule to give an account of no poison without an exact description of its properties, and without ascertaining by experiment the effects which it produces on animals. If we were disposed to find fault we should blame the author for the too great number of parts into which he has subdivided each article. This has occasioned some repetition, and given rather a stiff appearance to the work. But when the novelty of many of the subjects treated of is considered, and the numerous mistakes respecting them still taught in the most recent books on medical jurisprudence, it was better to err on the side of minuteness and repetition than on that of omission.

M. Orfila divides poisons into six classes; namely, *corrosive*, *astringent*, *acrid*, *stupifying*, *narcotico-acrid*, and *septic*. The present volume, published in two parts, includes the first two of these classes.

The corrosive poisons consist of preparations of the following substances: mercury, arsenic, antimony, copper, tin, zinc, silver, gold, bismuth. It includes likewise the following substances; sulphuric acid, nitric acid, muriatic acid, phosphoric acid, fluoric acid, oxalic acid, tartaric acid; the caustic alkalies, barytes, lime; phosphorus; cantharides. The astringent poisons are confined to preparations of lead.

The principal poisonous preparation of mercury is *corrosive sublimate*, or *deuchloride of mercury*. The chemical properties of this salt are described at great length. It may be sufficient here to say, that it is a white, heavy substance, having an acrid taste, and soluble in about 11 times its weight of water. When heated it sublimes in a white smoke, which excites coughing; but has not an alliacious smell. If a plate of clean copper be exposed to this smoke it becomes tarnished, and when rubbed assumes a white colour. The solution of corrosive sublimate is precipitated brick red by an alkaline carbonate; yellow by a caustic fixed alkali and lime-water; white by ammonia; white by prussiate of potash; black by a hydrosulphuret; white by albumen. Corrosive sublimate, when swallowed in a considerable dose, (as 30 grains) acts with great violence, occasioning evacuations both upwards and downwards, and death very speedily ensues. M. Orfila has ascertained that white of egg, dissolved in water, and administered in considerable quantity, and as speedily as possible, constitutes the best antidote to this poison.

All the preparations of arsenic are violent poisons; but the most common state in which it is administered is that of arsenious acid, or white oxide of arsenic. Its properties, and the fatal effects which it produces when taken internally, are so well known, that it would be superfluous to describe them here. When arsenic is swallowed in a state of solution, sulphureted hydrogen taken soon after is an efficacious antidote. But this poison is usually administered in a solid state, and in that case the antidote is totally inefficacious. The proper treatment is to endeavour to get the poison out of the stomach as speedily as possible, by vomiting. Great quantities of hot water, having some sugar or mucilaginous matter dissolved in it, should be swallowed, and vomiting excited, if it does not take place spontaneously, by introducing the finger, or a feather, into the throat. By persisting in this treatment many persons poisoned by arsenic have recovered.

Of the antimonial preparations there is scarcely any likely to be administered, or to be taken internally, so as to produce death, except tartar emetic, which is a triple salt composed of tartrate of potash and protartrate of antimony united together. This salt is usually administered as an emetic, and it generally acts in that way with great violence. When vomiting does not take place it acts as a poison, occasions violent spasms of the œsophagus and neck, which prevent the possibility of swallowing. When it is administered to dogs, and their œsophagus is tied up so as to prevent vomiting, the consequence is death. When a person is poisoned with tartar emetic, our object should be to produce vomiting, and for this purpose warm water is the most efficacious remedy. The decoction of yellow bark likewise, proposed by Berthollet, is of service, when administered in such quantities as to decompose the salts.

Perhaps no poisons are so frequent as the preparations of copper. This metal is used for so many purposes connected with the preparation of food, it is so easily oxidized, and all its compounds are of so deleterious a nature, that many instances must occur of injurious effects from it. The most common preparations of copper likely to be applied as poisons are verdigris, acetate of copper, sulphate of copper, nitrate of copper, muriate of copper, and copper dissolved by fat. The taste of all these preparations is exceedingly disagreeable; but they may be mixed in small quantities with food without being perceived. The preparations of copper occasion violent colics, vomiting, prostration of strength, and death. The best antidote is sugar, either swallowed solid, or dissolved in water. It should be taken both ways, and in considerable quantities. The liquid induces vomiting, and thus gets rid of a portion of the poison.

The only preparation of tin likely to be used as a poison is the muriate which is used in considerable quantities by the dyers. Its taste is exceedingly nauseous. It occasions violent colics, vomiting, and death. The best antidote is milk, which, when drank in considerable quantity, seldom fails to cure the patient, by decomposing the salt and removing all the disagreeable symptoms.

Zinc is so little employed for culinary purposes that it is but rarely that it can act as a poison. The sulphate of zinc, however, is so common a salt, that it has been often administered in considerable quantities by mistake. It is by no means a dangerous poison; for it acts speedily as an emetic, and is thrown out of the system before it has time to produce bad effects. The business of the physician is to promote this effect by making the patient swallow considerable quantities of warm water. Milk also, which decomposes the salt, may be administered with advantage.

Nitrate of silver is well known as one of the most corrosive salts employed in pharmacy. When introduced into the stomach it soon occasions death, by corroding that organ, and bringing on gangrene. When injected into the veins, even in very small quantities, the animal dies almost immediately. The antidote to this poison is common salt dissolved in water, which decomposes the nitrate of silver, and forms an insoluble chloride, which produces no injurious effects upon the animal economy.

Gold is not likely to be administered as a poison. When this metal is dissolved in nitro-muriatic acid it forms a salt, which acts more violently on the animal economy than corrosive sublimate. The symptoms are similar, except that the salt of gold does not produce the same effects upon the mouth and gums. No antidote against this poison is known. The object of the physician must be to get it thrown out of the stomach as speedily as possible by vomiting. I think it very probable that a solution of sulphate of iron would destroy its deleterious effects, by decomposing the salt, and throwing down the gold in the metallic state.

The nitrate of bismuth, and the white pigment for the face, known by the name of pearl white, which is a preparation of bismuth, act with considerable energy as poisons when introduced into the stomach. The best antidotes against these deleterious preparations are milk and mucilaginous liquids, swallowed in considerable quantities.

Sulphuric acid has sometimes been swallowed by mistake, and sometimes taken by persons who wished to destroy their lives. The violence of its action on animal substances is well known. The mouth, the œsophagus, and the stomach, are speedily corroded by it, and their functions destroyed; the consequence is death, attended with the most excruciating pain. From the experiments of M. Orfila it appears that the best antidote against this corrosive acid is calcined magnesia, and that if this substance be administered soon after the acid has been swallowed, it prevents death, and enables the patient to recover.

Nitric acid has been frequently swallowed in considerable quantities by unhappy persons who wished to destroy themselves. It is still more corrosive than sulphuric acid, acts with more violence, and produces dreadful pains. Magnesia is also the best antidote against this poison, and if administered very speedily, it may even save the life of the patient.

Muriatic acid, though it cannot be exhibited in so concentrated a state as sulphuric and nitric acids, produces the same deleterious effects when taken internally, and speedily occasions death, attended with the same dreadful symptoms. Magnesia is likewise the best antidote against this acid.

Phosphoric and fluoric acids, sulphurous acid, phosphorous acid, are all likewise poisonous; but as the chance of their being introduced inadvertently into the stomach is not great, it does not seem necessary to dwell upon them. M. Orfila likewise ranges oxalic and tartaric acids among poisons; but he gives no instance of their deleterious effects.

Caustic potash and soda are not less corrosive than the concentrated acids; hence, when introduced into the stomach, they act with equal violence, and very speedily destroy life. The best antidote is vinegar, administered in such quantity as to neutralize the alkali.

Ammonia likewise acts with considerable violence upon the animal economy when swallowed, and produces convulsions and death. Vinegar answers equally well as an antidote against ammonia as against the fixed alkalies.

Barytes, carbonate of barytes, and muriate of barytes, are known to act as violent poisons when introduced into the stomach. The effects which they produce are similar to those produced by the other corrosive poisons. An alkaline sulphate by converting the barytes into an insoluble sulphate, which does not act upon the human body, is the best antidote against this poison.

Lime is not a very energetic poison, yet when swallowed in considerable quantities, it destroys life by the inflammation which it induces in the stomach. The same mode of treatment answers for lime that was recommended in the case of poison by the fixed alkalies.

Phosphorus, when introduced into the stomach, always proves fatal. It is gradually converted into phosphorous and phosphoric acids, which corrode the stomach and intestines, and produce inflammation. The object of the physician should be to throw the phosphorus out of the stomach as speedily as possible by means of an emetic. When the phosphorus has been introduced in the state of extreme division it is useful to make the patient swallow large quantities of water, holding magnesia in suspension. The liquid, by filling the stomach, prevents the phosphorus from being readily converted into an acid, and the magnesia neutralizes any acid that may be formed.

M. Orfila introduces pounded glass and stone-ware among the number of poisons. He gives a number of cases in which these substances were swallowed without any inconvenience, and others in which they produced destructive effects. It is obvious that these substances act only mechanically. It must depend upon accident whether any of their sharp points wound any part of the stomach and intestines, or whether they make their way without inflicting any wound.

Cantharides, lytta vesicatoria, or *Spanish flies,* are a set of insects well known in the materia medica, as they constitute the essential ingredient in the common blistering plaster. Cantharides, according to the analysis of M. Robiquet, contain a variety of different substances; but the most important is a white substance, having the form of small crystalline plates, insoluble in water, soluble in boiling alcohol; but is deposited on cooling in small crystalline plates like spermaceti. It is soluble in oils. It possesses the blistering property in great perfection, and is the only substance in cantharides that has it.

The effect of cantharides when taken into the stomach in any quantity is well known. It produces a most furious satyriasis, which usually terminates in gangrene and death. No antidote against this formidable poison has been hitherto discovered.

All the preparations of lead are poisonous; but those most likely to be taken into the stomach are the oxides of lead, white lead, litharge, and sugar of lead. The water near lead mines, in which the galena is washed, is usually injurious to the health, in consequence of particles of that substance which it holds in suspension. The fumes of lead prove no less injurious to those who are exposed to them.

Lead shows its deleterious effects in those who are exposed to its action; but slowly. Obstinate costiveness and violent colics, known by the name of colica pictonum, first attack the patient. This is followed by paralysis and death. It appears from the experiments of M. Orfila, that sulphate of magnesia acts as an antidote against acetate of lead. An insoluble sulphate of lead is formed, which does not injure the animal economy, and the acetate of magnesia acts merely as a purgative. The common method of treating persons poisoned by lead is by a course of purgative and emetic medicines, which seldom fail to cure the patient.

In an appendix M. Orfila gives us a set of experiments on iodine, introduced into the stomach of animals. In small quantities it acts as an exciter. When administered to the amount of about half an ounce it occasions death, if the animal be prevented from vomiting, gradually corroding the stomach and intestines. When taken in larger quantities it destroys life even though the animal be allowed to vomit.

In another appendix M. Orfila shows by experiment, that charcoal powder is not an antidote against corrosive sublimate and white oxide of arsenic, as had been advanced by M. Bertrand.

ARTICLE XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, the 6th of April, a paper by Mr. Knox was read, on the coloured rings formed when a flat plate of glass is pressed against a convex lens. Mr. Knox conceives that the reason why neither Sir Isaac Newton, Dr. Herschell, nor any other philosopher, was able to give a satisfactory explanation of these coloured rings, was, that they were not acquainted with all the phenomena. He made his experiments according to the method pointed out by Dr. Herschell in his paper on the subject published in the Phil. Trans. for 1804. Mr. Knox described a great many new phenomena which he observed, and of which it is scarcely possible to give an idea without the assistance of figures. He found the phenomena the same when the experiments were tried in vacuo as in the open air. Nor did the introduction of water between the plates alter the phenomena much. Hence he conceives that the rings are not owing, as Newton supposed, to the film of air between the plates. He conceives them to be derived from the reflection of the surface of the glass next the film of air. The changes induced by the passage of the ray from one medium into another may occasion such refractions as to collect together the different bundles of coloured rays so as to produce the coloured rings.

On Thursday, the 13th of April, a paper by Major Rennell was read, stating further proofs in confirmation of the existence of a current setting upon the Scilly Islands, in the chops of the Channel. He adduced three proofs that there exists a current running east along the north coast of Spain. The French navigators are aware that there is a current which sets north along the west coast of France, and it is obviously the Spanish current which has received a northerly direction, from the position of the land. All the sand and alluvial matter which is brought into the Bay of Biscay by the Garonne, the Loire, and the other rivers which empty themselves into the sea on the west coast of France, is found on the north side of the mouths of the respective rivers, and not on the south, a circumstance which can be occasioned only by a northerly current. He brought several facts showing that a northerly current exists about lat. 49° at the mouth of the Channel, and rendered it probable that it flows also westerly, as well as north. This current flows at different times with different velocities, and this he assigned as the probable reason why it was not discovered sooner.

There is a current likewise which flows east along the south coast of Ireland, and meeting with the first described current, flows northward into St. George's Channel, and moves in the direction to Cardigan Bay. This current is the cause why ships are so frequently driven into that bay. There is a current which runs up along the west coast of Ireland, turns east along the north coast, and then flows south certainly as far as Dublin, and probably further. There is another current that flows north along the west coast of Scotland, bends round the northern part of the island, and flows south along the east coast of Great Britain as far as Harwich, where it meets with the current in the English Channel. These produce a current north-east along the coast of Flanders and Holland; it then proceeds north along Jutland, receives the current coming out from the Baltic, proceeds to the Naze of Norway, and then runs north along the coast of that country.

On Thursday, the 20th of April, a paper by Sir Humphry Davy on a combination of iodine and oxygen, was read. The author in a former paper had given an account of several unsuccessful attempts to form this compound. It occurred to him that if euchlorine gas (*oxide of chlorine*) were made to act directly on iodine, the result might be more fortunate. Accordingly he caused a current of euchlorine gas, dried by passing through muriate of lime, to act upon iodine. A combination took place. When the solid body formed was exposed to a moderate heat, chloriode was driven off in the state of vapour, and an oxiode, or compound of oxygen and iodine, remained behind. This substance is solid, of a white colour, and considerable specific gravity, as it sinks rapidly in sulphuric acid. Its taste is astringent. It dissolves rapidly in water, and forms a colourless solution, which has acid properties, and which the author calls oxiodic acid. This liquid first reddens vegetable blues, and then destroys them. Other colours it converts into yellow. When the oxiode is exposed to a heat rather below the boiling point of olive oil, it is decomposed, oxygen gas being driven off, and iodine left behind. Sir H. Davy, from various experiments, made however on a small scale, considers this compound as composed of one atom of iodine and five atoms of oxygen. If we reckon the weight of an atom of iodine 15·621, and that of an atom of oxygen 1, this will give us the oxiode composed of 15·621 iodine and 5 oxygen, or of

Iodine	75·75
Oxygen	24·25
	100·00

Oxiodine has the property of combining with the different bases, and forming oxiodes, which the author describes. The oxiodic acid likewise combines with the alkalies, earths, and oxides, and forms a class of salts, which he distinguishes by the name of oxiodates.

This substance has the remarkable property of combining likewise with the different acids, and of forming solid compounds, which for the most part crystallize. When dropped into sulphuric acid, a solid substance falls, which, when heated, melts, and assumes, on cooling, the form of yellow-coloured rhombs. According to Sir H. Davy's experiments, this compound is composed of 20 oxiode and 8 sulphuric acid. He considers it as a hydrate. If we suppose it a compound of one integrant particle of oxiode and one integrant particle of sulphuric acid, it appears from the above analysis that there may be present in it two integrant particles of water. The oxiode combines also with phosphoric acid, phosphorous acid, nitric acid, and oxalic acid. These combinations are probably all hydrates.

The oxiodic acid dissolves gold and platinum. When heated, the water is driven off, and the acid at last remains in the state of a thick paste. This paste is a hydrate of oxiodic acid. Sir H. Davy tried to obtain a compound of oxygen and iodine containing a less proportion of oxygen than the above-described oxiode; but his attempts were unsuccessful. The supposed combination of oxygen and iodine described by Gay-Lussac was a compound of oxiode and sulphuric acid, and contained likewise some barytes.

LINNÆAN SOCIETY.

On Tuesday, the 4th of April, a paper by Dr. Leach was read on the classification of the insects called notonectides.

On Tuesday, the 18th of April, a paper by the Rev. P. Keith was read, on the ascent of the sap in trees. Mr. Keith took a view of the different hypotheses hitherto proposed to account for the ascent of the sap, and showed that they were all inadequate to explain the phenomena. One of the latest of these hypotheses is that of Mr. Knight, who conceives that the ascent of the sap may be owing to what is called the *silver grain* of the wood. To refute this hypothesis, it is only necessary to mention that some of the tallest plants known have no silver grain whatever. Yet it is obvious that the sap ascends in them as well as in other plants. Mr. Keith considers the hypothesis of Saussure, that the sap is forced up by the contraction of the vessels, as with certain modifications, most likely to be true.

GEOLOGICAL SOCIETY.

March 17, 1815.—A communication was read from the Woodwardian Professor; the object of which paper is to describe a remarkable variety of geode, several individuals of which were lately met in digging a well at Oakhampton, Devonshire. They occurred in a bed of clay about ten feet deep. Their figure is nearly that of a compressed spheroid, and each ball consists of a nucleus of ochreous oxide of iron enclosed within a shell of a cavernous structure, the shells of which are externally so regular that the mass might easily be mistaken for a fossil madrepore.

With regard to the mode in which this and similar bodies have been formed, the Professor suggests that the deposition of the beds in which they are formed might have been attended with effervescence, and that this spheroidal figure and cavernous structure might have been occasioned by gas uniformly distending, and at length escaping insensibly through the cellular crust by which it had been confined.

The reading of Mr. Horner's paper on the south-western part of Somersetshire was continued.

April 7.—A short notice from Mr. Horner on the locality of some specimens from the island of Tino, presented by him to the Society, was read; also a communication from the Woodwardian Professor, supplementary to his former, on the Cambridgeshire strata.

This paper furnishes a new locality of the flattened Headington oyster, it having been lately found forming a strong bed at Willham, about six miles north of Ely, in the Fen level, a position which ascertains its geological situation to be below the chalk. Another remarkable appearance is stated to occur at Reche, not far from

Swaffham, on the edge of the fen. This place has for ages supplied the surrounding country with the chalky lime-stone called clunch, in consequence of which extensive excavations have been formed, which have laid bare a large mass of a lenticular shape imbedded in the clunch. The greatest thickness of this mass is about eight feet, and its length and breadth from 30 to 40 yards. It is comprised of kidney-shaped masses, about the size of a walnut, of ochre-yellow lime-stone, covered superficially with a thin coat of green oxide of iron. It contains a few *spines* of an echinus completely converted into spar, and also vegetable impressions seemingly belonging to plants of the tribe of gramina.

At this meeting the reading of Mr. Horner's paper on the south-western part of Somersetshire was concluded.

The most elevated ground in the district here described is Exmoor Forest and the Quantock Hills. This tract is composed for the most part of a grey-wacke formation; the strata of which present more or less coarsely-granular mixtures of quartz and clay, sometimes considerably calcareous, alternating with slaty beds, which often are not to be distinguished by external characters from primary clay-slate. Within these latter beds are included thick short beds of lime-stone full of madrepores, and containing veins and nests of grey copper malachite and hæmatite. In the ground of Hestercombe occurs a wall (probably a vein) of fine-grained grey granite; the slate rises up towards it at a very high angle, is considerably indurated, and at the plane of junction appears to be more or less penetrated by the granite.

Where the hills of grey-wacke sink down into the lower country their sides are covered with beds of conglomerate passing into red sand-stone, which extend to a considerable distance. These beds appear to consist of the same materials as the grey-wacke formation, but decomposed to a considerable degree. They are traversed by veins of calcareous spar, sometimes of so great a thickness as to be quarried for lime-stone.

The conglomerate, with its accompanying sand-stones, is covered in many places by a red argillaceous sand-stone, containing a variable proportion of calcareous matter, but being principally characterized by spots and stripes of a greenish-grey colour. It is of an uniformly fine texture, never contains any fragments either angular or rounded, is in places traversed by veins of gypsum, and appears to be the same rock as that in which the salt-*beds* of Cheshire and Droitwich are situated.

To this rock succeeds the strata of Lyas lime-stone, which with their accompanying beds of slate-clay are sometimes seen distinctly resting on the red gypseous rock, and sometimes in very broken and disturbed stratification appear to alternate with it.

On the sea coast about three miles westward of the river Parrett, in the cavities formed by the curvature of the Lyas strata, is a stiff blue clay, covered with peat, in which are imbedded trunks of trees, often of great size, and with their lateral branches still attached to them. These remains of an ancient forest extend to an unknown distance into the sea.

Article XIII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

The Summer Courses of Lectures on the Theory and Practice of Physic, by Dr. Roget, and of Materia Medica and Medical Jurisprudence, by Dr. Harrison, will commence, as usual, in Windmill-street, on the first week in May. The Lectures on Chemistry will, in consequence of Dr. Davy's absence from town, be given during the summer by Dr. Granville.

Dr. Clutterbuck will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Friday, June 2, at ten o'clock in the morning, at his house, No. 1, in the Crescent, New Bridge-street, Blackfriars, where further particulars may be had.

II. *Gas Lights.*

In answer to M., who puts some queries on the subject of gas lights in the last number of the *Annals of Philosophy*, p. 313, I have to observe that, in all my experiments on carbureted hydrogen gas I never was able to produce an explosion by firing any mixture of it whatever with common air. It merely burnt rapidly. Such a rapid combustion on a large scale I am sensible would produce an explosion. When mixed with oxygen gas, it will not fire unless it bear a certain proportion to the oxygen gas. The oxygen gas must amount at least to 105 measures (supposing we take 100 measures of carbureted hydrogen gas), and no explosion takes place whenever the oxygen amounts to 227 measures. From these facts, I infer that whenever 11 parts of common air and one part of carbureted hydrogen are mixed together, the mixture will explode when kindled; but if the carbureted hydrogen exceed $\frac{1}{6}$th of the common air, it will no longer be capable of exploding. All proportions between $\frac{1}{11}$th and $\frac{1}{6}$th will explode.

The gas produced by the distillation of pit-coal consists almost entirely of carbureted hydrogen. Hence I conceive that the preceding observations apply to it correctly; of course coal gas, when collected in reservoirs, never can explode unless it be mixed with at least six times its bulk of common air. This I think never can happen except from an unaccountable and culpable negligence of those who are employed. If the reservoir is not air-tight, if it be filled with coal gas, and the pressure by which it is made to sink in the water as the gas is expended be taken off, I can easily conceive that after an interval of a day or two (according to the size of the lack), five-sixths of the gas may make its escape, and be replaced by as much common air; for as coal gas is much lighter than common air, it will always make its escape when it can. I am persuaded that the explosion at Birmingham, and some others which I have heard of, were produced in this manner. But as nothing is easier than to ascertain whether the reservoir be air-tight, I conceive that with common precaution gas lights may be used with as much safety as any other light whatever.

As to the coal that answers best, it is undoubtedly that variety which contains the greatest quantity of bitumen. Newcastle and Wigan coal will probably answer better than any other coal in the island, except some of the Fife coal, which is absolutely of the same quality.

With respect to the degree of pressure requisite to make it issue with sufficient velocity from the mouth of the pipes, I do not know that any accurate experiments have been made to determine it, nor do I believe that much nicety is necessary. Some Gentlemen have expressed doubts on the subject, because Mr. Wilkinson was not able to force an efficacious draught of air through a pipe a few hundred feet in length; but the cases are by no means parallel. Mr. Wilkinson required a current of considerable velocity; but no such velocity is requisite for the coal gas. Besides, the water generated by the combustion of the coal gas at the extremity of the tube must occasion a diminution of pressure, which will serve to regulate the issue of the coal gas from the reservoir.

III. *Toads found in Rocks and Trees.*

A correspondent at Bristol, I. B. I., has proposed some queries relative to the many stories in circulation respecting toads found in the middle of solid rocks. There can be no doubt that toads have been frequently found in such positions, though in no one case that I have seen has it been ascertained that the animal was completely excluded from the external air. I have myself conversed with workmen who had found toads both in coal-pits and in quarries; but they were never able to bring decisive proof that the animal was completely surrounded by the coal or the stone. It is generally observed that when a toad is found in this position the creature dies very soon after being brought out of its lurking hole. This seems to me a proof that the animal, if not entirely excluded from the air, must have been at least nearly so; for the sudden death can scarcely be ascribed to any thing else than the change of situation.

IV. *On the Focal Powers of the Eye.* By Dan. Pring, Surgeon.

It has been presumed from the supposed resemblance of the eye to some optical contrivances, that it is necessary the former should possess a variable focal power, in order to account for the capability of viewing objects at various distances. But this supposition appears to be founded upon a false analogy. The eye sees with a distinctness which agrees with the distance of the object: a result which can be determined only by the faculty of vision itself, without any correspondent variety of the focal power. That the focal powers of the eye undergo no change in order to produce vision at different distances, appears from this fact, namely, that we are enabled to see a great variety of objects *at all distances*, within a range of perhaps from three yards to three miles *at the same time*. A thousand objects may be interspersed in this range. Can there, then, exist a correspondent number of distinct focal powers in the eye *at the same time?*

The eye requires *a determinate focal power*: its only movements are for the purpose of regulating the axis of vision: and perceptions are formed according to the relation which subsists between light at various approximations to a focus, and the faculty of vision allied with the retina.

Bath, *April* 4, 1815.

V. *Proposed Road over Hounslow Heath.*

(To Dr. Thomson.)

SIR,

AN Act of Parliament has passed for the inclosure of Hounslow Heath, and the commissioners have already begun to act upon it. It may not, however, be too late to induce them to lay down one of their roads in the line on which Gen. Roy measured his base. The plan seems unobjectionable, and it certainly would be attended by circumstances which make the execution of it highly desirable. The suggestion of it in the *Annals* can at least do no harm, and will oblige

A CONSTANT READER.

VI. *On Mr. Lockhart's Imaginary Cube Roots.* By Dr. Tiarks.

(To Dr. Thomson.)

SIR,

Having seen in the last number of your *Annals* a paper by Mr. Lockhart, which contains this most extraordinary assertion, that every cubic equation has more than two imaginary roots, I beg leave to state to you that the imaginary expression which Mr. L. supposes to be a root of 4, different from the two well-known roots, is nothing but a different form of one of them.

Mr. L.'s expression is, $-\frac{3+\sqrt{-3}}{2}+\sqrt{-\left(\frac{13}{2}-\frac{3}{2}\sqrt{-3}\right)}$. Now it will be easily seen that $-\left(\frac{13}{2}-\frac{3}{2}\sqrt{-3}\right)$ is the square $-\left(\frac{1+3\sqrt{-3}}{2}\right)$. The above expression, therefore, is, $-\left(\frac{3+\sqrt{-3}}{2}+\frac{1+3\sqrt{-3}}{2}\right)=-(2+2\sqrt{-3})$, which is really one of the two imaginary roots of 4, $-(2 \pm 2\sqrt{-3})$ resulting from the equation $x^2 + 4x + 16 = 0$.

I am, Sir, your obedient servant,

5, Bateman's Buildings,
Soho-square.

J. L. Tiarks.

VII. *Another Communication on the same subject.*

(To Dr Thomson.)

SIR,

My attention was arrested by the seventh article of scientific intelligence in your last number (p. 315). Some mathematicians have denied the universality of the doctrine of there being as many roots to an equation as it has dimensions, but none have been able to maintain that there are more. I therefore examined Mr. Lockhart's proof with some attention; and I conceive that he cannot take it ill if I endeavour to point out the source of his mistake. I think, likewise, that you will be indebted to me for doing so; since the "method for approximating towards the roots of cubic equations belonging to the irreducible case," has justly given some weight to the author's opinions; and you must be desirous of not being the means of propagating an error which can only be supported by the authority of his name.

I must begin by laying down that $-\frac{3 + \sqrt{-3}}{2} + \sqrt{-(\frac{13}{2} - \frac{3}{2}\sqrt{-3}}$ is *not* a cube root of 64, but of 8. To show this in the simplest manner, we will substitute $\frac{1}{2} + \frac{3}{2}\sqrt{-3}$ in the place of $\sqrt{-(\frac{13}{2} - \frac{3}{2}\sqrt{-3}}$, for these two quantities may easily be shown to be equal by the rule for extracting the roots of binomial surds; and then $-\frac{3 + \sqrt{-3}}{2} + \frac{1}{2} + \frac{3}{2}\sqrt{-3} = \frac{-2 + 2\sqrt{-3}}{2} = -1 + \sqrt{-3}$, which, when cubed, will be found to make up 8. But it may be asked how a mathematician so well acquainted with algebraical processes, and especially with cubic equations, can have made such a mistake, and in what part of his reasoning the fallacy lies? This question I think admits of a complete answer; for the error will be found in his manner of bringing out the value of $(-2 + 6\sqrt{-3})\sqrt{-(\frac{13}{2} - \frac{3}{2}\sqrt{-3}}$. It is perfectly clear that $a\sqrt{b}$ is equal to the square root of $a^2 b$; but it escaped Mr. L. that by squaring his quantity he introduced an ambiguity, since the square root of $a^2 b$ is $\pm a\sqrt{b}$; and in this instance he ought to have taken the negative instead of the positive root; the value then would have been $36 - 28 = 8$, instead of $36 + 28 = 64$. To show that this is so, we have only to take the value above assigned to $\sqrt{-(\frac{13}{2} - \frac{3}{2}\sqrt{-3}}$; and then we shall find that $(-2 + 6\sqrt{-3}) \cdot (\frac{1}{2} + \frac{3}{2}\sqrt{-3}) = -28$.

There are some particulars in your last number which would not deserve a separate communication, and on which I may yet take this opportunity of offering some short remarks. In your account of Count Rumford you have omitted to mention the memoirs which he published abroad, and the medal which he entrusted for distribution to the Royal Society. To the last query of M. (p. 314), it may be answered that he will find the subject mentioned at p. 31 of Smith's Optics, and at p. 148 of Dr. Jurin's Essay on Distinct Vision (annexed to that work); and that a number of curious experiments, which give greater precision to the inquiry, will be found in Harris's Optics, and in a paper of Dr. Herschell's in the Phil. Trans. for 1786 (vol. lxxvi.).

N. R. D.

VIII. *Effect of Sulphuric Acid on Agates.*

What can be the reason that agate heated, or rather boiled, in concentrated sulphuric acid, becomes in its different layers differently coloured; the white strata becoming milk-white, and the greyish ones as black as pitch? I heard this from Mr. Banks, in Bath, and would not believe it; but Mr. Banks was so good as to make the experiment in my presence, and I was convinced of the fact. Saxon agate is more affected than the Scotch is: half an hour or one hour is generally sufficient for producing the effect. The colours penetrate to a considerable depth. I think this fact fully merits the attention and investigation of chemists and mineralogists.

Glasgow, March 15, 1815. J. Hamel, M.D.

IX. *Constituents of the Ribes Grossularia,* (*Green Goosberry.*)

We know from Scheele that the juice of this fruit contains citric and malic acids. Dr. John has lately subjected it to a more detailed analysis. This juice has a greenish, and somewhat thick consistence; but it does not gelatinize when exposed to the air. John could find in it hardly any traces of sugar, and therefore is disposed to doubt the possibility of converting it into wine; but this is often and successfully put in practice in this country. Indeed the taste of this gooseberry is very distinctly sweet. The following are the constituents found in this juice by John.

Much water.
Uncrystallizable sugar.
Supercitrate of potash.
Supermalate of potash.
Supercitrate of lime.
Supermalate of lime.
A little resin.
Prunin or cerasin.*
Insoluble modified gum.
A salt with base of magnesia.
Traces of phosphates of lime and magnesia.
Trace of muriate of lime?
A little phosphate? of iron.
Ammonia, probably combined with citric and malic acids.
Fibrin.

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* This is a substance similar to gum in appearance; but it does not dissolve in water, only swelling up and becoming gelatinous in that liquid.

X. *Constituents of Angelica or Changelica.*

This plant, one of the greatest ornaments of cold countries, has been analyzed by John. The following are the constituents which he obtained from 300 parts of the dried plant.

Colourless and very volatile oil	
Gum......................................	100·5
Inulin	12
Bitter extractive	37·5
Sharp tasted resin	20
A peculiar substance, soluble only in potash....	22
Woody fibres	90
Water and loss	18
	300

The earthy constituents were

Phosphate of lime.
Phosphate of iron.
Phosphate of magnesia.
Silica?

Article XIV.

New Patents.

Robert Dickinson, Great Queen-street, London; for certain improvements in the art of sadlery. Nov. 28, 1814.

Robert Dickinson, Great Queen-street, London; for certain improvements in the manufacture of barrels and other packages made of iron or other metals. Dec. 10, 1814.

Robert Salmon, Woburn, Bedford; for improved movements and combinations of wheels for working of cranes, mills, and all sorts of machinery, either portable or fixed. Dec. 10, 1814.

Edward Glover, Penton-place, Walworth, Surrey; for an apparatus for drawing or extracting bolts, nails, &c. and for various other useful purposes. Dec. 10, 1814.

Henry Julius Winter, Dover; for a method of giving effect to various operating processes. Dec. 12, 1814.

John Francis Wyatt, Furnival's Inn, engineer; for a new kind of bricks or blocks, one of which is particularly adapted for the fronts of houses and other buildings, giving to them the appearance of stone; another is applicable to a new method of bonding brick-work; also a new kind of blocks or slabs for paving floors, and facing or lining walls, instead of ashler, which will resemble marble or stone, and which may also be applied to steps or stairs, and other parts of buildings. Dec. 15, 1814.

Joseph C. Dyer, of Boston State, America, now residing in

Camden Town, Middlesex; for certain additions to, and improvements on, machinery to be made and applied in manufacturing cards for carding wool, cotton, silk, and tow, and other fibrous materials of the like description. Communicated to him partly by a foreigner residing abroad. Dec. 15, 1814.

JAMES SMITH, Newark-upon-Trent; for a self-acting sash fastening. Dec. 20, 1814.

WILLIAM EVERHARD, Baron von Doornich, Sun-street, Bishopgate-street, London; for improvements in the manufacture of soap. Dec. 20, 1814.

JOHN VALLANCE, jun. Brighthelmstone; for an apparatus and method of so constructing and securing brewers' vats or store-casks as to prevent the vat falling to pieces, or even breaking, though every one of the hoops on it should be broken asunder, and consequently preventing the liquor from being lost; and also for preventing the loss of liquor, even if a cock or all the cocks of the vat should be broken off. Dec. 20, 1814.

ROBERT DICKINSON, Great Queen-street, London; for certain improvements in implements applicable to the purposes of navigation, namely, an improvement or improvements in the ship's nunbuoy and beacon-buoy. Dec. 20, 1814.

EDWARD JORDEN, Norwich, and **WILLIAM COOKE**; for an apparatus for the detection of depredators, which they denominate *The Thieves' Alarum.* Dec. 24, 1814.

FREDERICK KOENIG, Castle-street, Finsbury-square; for certain further improvements in his method of printing by means of machinery. Dec. 24, 1814.

JOHN WHITE, New Compton-street, Soho; for a method of making candles. Dec. 27, 1814.

JOSEPH HARRIS, Shire-lane, Middlesex; for an improvement or improvements in the necessaries of clothing used for the military in general. Jan. 4, 1815.

JOHN CATTLER, Great Queen-street, Lincoln's Inn-fields; for certain improvements applicable to fire-places, stoves, &c. Jan. 6, 1815.

CHRISTOPHER DIHL, Brewers-street, Golden-square; for a method or means of making a mastic cement or composition, which he denominates *Dihl's Mastic.* Jan. 6, 1815.

JAMES COLLIER, Grosvenor-street West, Pimlico; for an apparatus, machine, or instrument, intended to be called a *Creopyrite*, by means of which power will be very economically obtained, and advantageously applied to the raising of water, and other useful purposes. Jan. 16, 1815.

FREDERICK Marquis de **CHABANNS**, Thayer-street, Manchester-square; for a method of extracting from fuel a greater quantity of caloric than hath hitherto been acquired, and applying it to the purpose of warming the room in which the operation is conducted, and also other rooms by one single fire. Jan. 16, 1815.

ARTICLE XV.

METEOROLOGICAL TABLE.

1815.	Wind.	Barometer.			Thermometer.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
3d Mo.										
Mar. 2	Var.	30·22	30·17	30·195	48	35	41·5		8	☾
3	N W	30·20	30·17	30·185	49	37	43·0			
4	S W	30·17	30·11	30·140	54	41	47·5			
5	N W	30·17	30·06	30·115	55	40	47·5			
6	W	30·17	29·90	30·035						
7	S W	29·90	29·35	29·625	52	38	45·0		—	
8	W	29.50	29·35	29·425	52	32	42·0		·45	
9	N W	29·46	29·16	29·310	49	34	41·5	·30	·15	
10	W	29·32	29·16	29·240	48	29	38·5		6	
11	S W	29·48	29·32	29·400	47	30	38·5		—	○
12	S	29·32	28·86	29·090	49	35	42·0		·39	
13	N W	29 52	28·86	29·190	52	39	45·5		·10	
14	S W	29·96	29·52	29·740	49	32	40·5		5	
15	Var.	29·96	29·68	29·820	55	37	46·0		·19	
16	S W	29·83	29·68	29·755	62	43	52·5			
17	N W	29·97	29·83	29·900	62	42	52·0	·35		
18	N W	29·99	29·88	29·935	55	46	50·5		—	☽
19	N W	29·91	29·88	29·895	57	45	51·0		·13	
20	W	29·91	29·75	29·830	60	41	50·5			
21	S W	29·75	29·54	29·645	59	45	52·0		6	
22	S W	29·54	29·31	29·425	61	45	53·0		6	
23	W	29·42	29·14	29·280	59	40	49·5		·15	
24	S W	29·42	29·25	29·335	57	40	48·5	·35	8	
25	S W	29·70	29 25	29·475	56	31	43·5			
26	S W	29·70	29·42	29·560	53	46	44·5		—	●
27	S W	29·46	29·34	29·400	60	49	54·5		·12	
28	S W	29·95	29·46	29·705	60	44	52·0		·26	
29	S	29·93	29·77	29·850	62	43	52·5			
30	S W	29·93	29·88	29·905	59	43	51·0			
31	Var.	29·88	29·65	29·765	73	46	59·5	·43		
		30·22	28 86	29·672	73	29	47·44	1·43	2·33	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Third Month.—2. Small rain at intervals. 3. Misty morning: fine day. 4, 5. *Cumulostratus.* 6. Fine day: *Cirri* appeared, much elevated, and coloured at sun-set. 7. Fine morning: p. m. cloudy and windy, with some rain: night very stormy. 8. Windy, wet, a. m.: showers by inosculation in the evening. 9. a. m. Hoar frost: turbid sky: rain: p. m. fair. 10. Snow early, after which various modifications of cloud, ending in showers of rain and snow, p. m. 11. Hoar frost: *Cirrostratus* and *Cumulostratus:* p. m. *Nimbi,* with large hail. 12. Dull misty day: at night very stormy, with rain. 13. a. m. Cloudy, with a gale at S. W., and rain at intervals: p. m. several dense *Nimbi,* thunder, hail, and hard rain: much wind, with distant lightning, at night. 14. *Cirrostratus* and haze: then *Cirri,* passing to dense *Nimbi:* gusts of wind, hail, and rain. 15. The barometer has risen, with an almost uniform motion, about an inch and a quarter in 36 hours; yet the air has not become clear: it should be observed, that there had been much previous depression: a wet forenoon, with a breeze at E.: p. m. *Cirrostratus:* at night much wind. 16. a. m. High wind at S. W., with *Cumulostratus:* fair and pleasant. 17. a. m. Much dew: *Cirrostratus,* with *Cirrocumulus:* the light clouds after sun-set beautifully tinted with lake and purple. 18. After a few drops, the *Cumulostratus* prevailed, followed by rain in the night. 19. Some rain, a. m.: then *Cumulostratus:* and at evening *Cirrostratus,* with a lunar corona. 20. a. m. Dew: a light veil of *Cirrostratus:* at evening, the clouds passed to the N. 21. *Cumulus,* beneath *Nimbiform Cirrus,* both elevated: about five, p. m. during the approach of a squall, the wind was very noisy among the branches (now covered with opening buds), producing an almost vocal modulation of sound: as soon as the trees became wet, this was exchanged for the usual hoarse noise, resembling that of the sea-shore. It is probable that the former effect requires a peculiar sonorous vibration in the branches, the effect of *close friction* by the air, which the interposition of water does not permit to take place. The night was boisterous. 22. Much wind: showers: two strata of cloud: borne very high, as for some days past. 23. Heavy squalls, with some hail in the showers: p. m. a singular combination of clouds in the E.: it was a *Nimbus,* with *Cumuli* adhering and entering at the flanks, while a very lofty columnar *Cumulus* shot up through the midst of the crown, and this again was capped with a small *Cirrostratus.* 24. Various clouds: squally, p. m. 25. The same: a brisk evaporation: at sun-set, *Cumulus* at a considerable height, inosculated with *Cirrus* above, after which two distinct *Nimbi* in the S., which went away eastward. 26. Driving showers: at evening a lunar corona, followed by much wind and rain at intervals. 27. Stormy: showers. 28. Fair. 29. Large *Cirri,* which passed chiefly to the *Cirrocumulus,* p. m. 30. Misty, a. m.: overcast, p. m.: little wind. 31. A very fine day: large *Cirri* formed alone at a considerable elevation, and passed in the evening to the N. W.: much dew followed.

RESULTS.

Winds Westerly.

Barometer: Greatest height........................30·22 inches;
Least...................................28·86 inches;
Mean of the period...................29·672 inches.

Thermometer: Greatest height...............................73°
Least...29°
Mean of the period...............................47·44°

Evaporation, 1·48 inch. Rain, 2·23 inches.

Tottenham, *Fourth Month,* 1, 1815. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1815.

ARTICLE I.

A Memoir on Iodine. By M. Gay-Lussac.

(*Continued from* p. 302.)

Hydriodate of Zinc.

THIS salt is easily obtained by putting iode into water with an excess of zinc, and favouring their action by heat, as I have already explained. I have frequently attempted, but always without success, to make this salt crystallize, because it is extremely deliquescent. Heat first deprives it of its water, then melts it, and sublimes it in fine prismatic crystals, similar to those obtained when antimony is oxidized. It is not decomposed by this operation, if performed in close vessels; but if air be admitted, iodine is disengaged, and oxide of zinc remains. When this hydriodate is dried, it does not differ from ioduret of zinc.

By taking the mean of three experiments, differing very little from each other, I find that ioduret of zinc is composed of

Iodine	100
Zinc	26·225

Consequently the hydriodate is composed of

Acid	100
Oxide of zinc	32·352

When a solution of hydriodate of potash or soda is mixed with a solution of metallic oxides, no precipitate is obtained with those of manganese, nickel, and cobalt, which proves that the hydriodates of these metals are soluble. Perhaps we may say that all the combinations of iodine with the metals that decompose water possess the same property.

On the contrary, the metals that do not decompose water have given me precipitates with the hydriodate of soda. The precipitate of copper is whitish-grey; that of lead, a fine orange-yellow; that of protoxide of mercury, greenish-yellow; that of peroxide of mercury, orange-red; that of silver, white; that of bismuth, chesnut-brown.

I consider all these precipitates as metallic iodurets, and with so much the greater reason, that the hydriodates of the very oxidable metals are changed into iodurets merely by drying them by a gentle heat. Now the force which has determined the insolubility of all these precipitates ought to be considered as much more energetic than a small change of temperature which is sufficient to convert a hydriodate into an ioduret.

It will not be useless, in order to settle our opinions respecting the nature of the combinations of the metals with sulphur, iodine, and chlorine, when in contact with water, to show the analogy which they have to one another.

Among the sulphurets, those only of the metals, which have a much greater affinity for oxygen than hydrogen has, are soluble in water, and may be considered, with some probability, as hydrosulphates. Such are those of potassium, sodium, barium, &c.

Though iron and zinc decompose water, they have not so superior an affinity for oxygen above hydrogen, that the united affinities of the metal for oxygen, and the sulphur for hydrogen, are stronger than that of the oxygen for hydrogen, and the metal for sulphur. I neglect here the affinity of the oxide for hydro-sulphuric acid, because it must be very weak relatively to the others. The metals which readily yield their oxygen to hydrogen will form *à fortiori* sulphurets, which will not decompose water, and which will be insoluble in that liquid.

When we compare the iodurets with the sulphurets, we must attend to this circumstance, that iodine has a stronger affinity than sulphur for hydrogen, and that from this there ought to result an augmentation of intensity in the forces which tend to decompose water.* We see, in fact, that all the metals which give soluble compounds with sulphur form equally soluble ones with iodine; and further, that the iodurets of the metals which decompose water possess the same property. As to the iodurets of the metals, which have less affinity for oxygen than hydrogen, they are insoluble, as well as their sulphurets.

Pursuing the same comparison with the chlorurets, we ought, according to the same principles, to find a greater number which are soluble than of the sulphurets and iodurets, because chlorine has a much stronger affinity for hydrogen than sulphur or iodine. This accordingly is the case. All the chlorurets of the metals which form soluble iodurets are likewise soluble, and besides them those of lead, bismuth, gold, platinum, the deutochlorurets of copper and mercury possess the same property.* We see, then, by the comparison which we have just made, that it is the most oxidable metals, and the radicles that have the greatest affinity for hydrogen, which have the greatest tendency to form combinations soluble in water, and which probably decompose it.

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* It may be objected, that if the forces which tend to decompose water have increased, because iodine has more affinity than sulphur for hydrogen, those which tend to prevent its decomposition have also increased, because iodine has more affinity than sulphur for potassium and the other metallic bodies. But we may suppose, with sufficient probability, that the first have increased in a greater ratio than the second.

I have attempted to decompose several hydriodates by acids in which the oxygen is very much condensed; but I have not obtained any satisfactory result. The hydriodate of strontian, and that of potash, treated by concentrated phosphoric acid, gave me very deep coloured hydriodic acid. Boracic acid produces no sensible decomposition, because it is too weak as long as there is any water mixed with it, and when there is none the hydriodate is changed into ioduret. Liquid hydro-chloric acid does not decompose the hydriodates, because it is more volatile than hydriodic acid; but in the gaseous state it decomposes the iodurets in an elevated temperature. I passed slowly through a glass tube containing ioduret of potash that had been melted, a current of hydro-chloric gas. There was no decomposition while cold. When the temperature was raised nearly to a red heat, I obtained hydriodic gas containing but very little hydro-chloric gas. With the iodurets of strontium and calcium, the decomposition takes place much better. This method may be employed with advantage in order to procure hydriodic gas.

Iodureted Hydriodates.

All the hydriodates have the property of dissolving abundance of iodine, and by this they acquire a deep reddish-brown colour. They keep it in solution by a very weak force: for they let it go when boiled, or when exposed to the air after being dried. The iodine does not change the neutral state of the hydriodates; and the reddish-brown colour of the solutions, similar to the other solutions of iodine, is a new proof of the weakness of the combination. We cannot compare these compounds to the sulphureted sulphites, in which the sulphur appears to act the part of an acid. They have rather the characters of a simple solution. I am aware that combination and solution depend upon the same force, and that it is difficult to draw a limit between them. But we may distinguish them from each other by defining a solution to be a combination in which there is no saturation of properties. At any rate, there is no inconvenience in adopting the expression *iodureted hydriodate* to denote the combination of iodine with a hydriodate, provided we entertain an exact idea of that combination.

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* The prochlorurets of copper and mercury are insoluble, while the deutochlorurets are very soluble. Though we may explain this difference on the hypothesis that chlorurets do not dissolve in water but in as much as they decompose it, these facts seem to me more favourable to the other hypothesis, that the chlorurets may dissolve in water without decomposing it. I have called the first combination of copper and mercury with chlorine *prochloruret*, because it corresponds with their protoxides; and the second *deutochloruret*, because it corresponds with the second degree of their oxidation.

Of the Iodates.

When speaking of the action of the alkaline oxides on iodine by means of water, we have shown that there form at the same time a hydriodate and an iodate, and we pointed out the method of separating them from each other. We may obtain the iodates of the other oxides either by double decomposition, or by saturating directly iodic acid, or the acid liquid from chloruret of iodine, which we have considered as a mixture of iodic and hydro-chloric acids, and which, at any rate, possesses exactly the properties of such a mixture.

There are but very few of the iodates that fuse on burning coals; that of ammonia is fulminating.

They are all soluble in hydro-chloric acid, with the disengagement of chlorine. The solution contains subchloruret of iodine.

Sulphurous and hydro-sulphuric acids decompose them, and separate the iodine. Chlorine does not decompose them. Sulphuric, nitric, and phosphoric acids, have no action on them at the ordinary temperature, except as far as they seize a portion of their base.

At a faint red heat all the iodates are decomposed. Some give oxygen only; others, oxygen and iodine.

They are all insoluble in alcohol of the sp. gr. 0·82.

Iodate of Potash.

I have only obtained it in small crystals, which have nearly the cubic form. It deflagrates on burning coals like nitre. It is not altered by exposure to the air. 100 parts of water at 58° dissolve 7·43 of this salt. It is decomposed at a temperature somewhat higher than is required by the chlorates. Oxygen is disengaged, and ioduret of potassium remains, which forms a neutral solution in water. It is easy to see that the residue is ioduret of potassium. If the metal were in the state of an oxide, iodate and hydriodate of potash would be formed on dissolving it in water, and sulphurous acid would occasion a precipitate of iodine. If, then, we wish to obtain a pure hydriodate by treating iodine with potash, we must evaporate the solution to dryness, and melt the residue. On redissolving it in water, we are certain to have only hydriodate; but there will always be an excess of base.

From various experiments on the decomposition of iodate of potash by heat, I have found it composed of,

Oxygen	22·59
Ioduret of potassium	77·41
	100·00

But from the composition of ioduret of zinc given above, namely, iodine 100, zinc 26·225, and admitting, according to my experiments, that 100 zinc combine with 24·41 oxygen, and according to Berzelius, that 100 potassium unite with 20·425 oxygen in order to be converted into potash, we find that ioduret of potassium is composed of

Iodine	100
Potassium	31·342

According to this proportion, the 77·41 of ioduret of potassium are composed of

Iodine	58·937
Potassium	18·473
	77·410

The 18·473 potassium take 3·773 oxygen to be converted into potash. There remain, then, 22·59 − 3·773 = 18·817 to acidify the iodine; so that iodic acid is composed of

Iodine	100
Oxygen	31·927

The ratio of oxygen to iodine is, then, 10 to 31·321. If we multiply this last number by 5, we have the ratio 10 to 156·605, which differs but little from the ratio formerly obtained, 10 to 156·21, from the hydriodate of zinc.

If we convert the iodine and oxygen into volumes, we find that iodic acid is composed of

Iodine	1
Oxygen	2·5

It is now easy to determine how much ioduret of potassium we ought to obtain, relatively to the iodate of potash which is formed at the same time, when iodine is dissolved in potash. Since 100 iodate of potash contain 22·59 oxygen, of which 3·773 belong to the potash, it is evident that the remainder 18·817 which belong to the iodine has been furnished either by the potash, which after giving it out has formed ioduret of potassium, or, which comes to the same thing, by the water the hydrogen of which formed hydriodate of potash; but 18·817 oxygen correspond with 92·127 potassium, and this last with 293·940 iodine. Therefore, for every 100 of iodate of potash there are formed 386·067 of ioduret of potassium; that is to say, five times as much as the iodate would give by its decomposition. This ratio may be deduced immediately from the ratio of the oxygen of the iodine to that of the potassium; for $\frac{18 \cdot 817}{3 \cdot 773} = 5$ very nearly.

If we wish to have the quantity of hydriodate of potash which corresponds to 100 of iodate, we must add to the 92·127 potassium 18·817 oxygen, and to the 293·940 iodine 2·497 hydrogen, which saturate the oxygen of the potassium. Thus we obtain 407·381 of hydriodate.

Iodate of Soda.

The iodate of soda crystallizes in small prisms, usually united in tufts. I have likewise obtained them in small grains seemingly cubic. They deflagrate on burning coals like nitre, at a heat somewhat below a red heat; but at the same time, a very small quantity of iodine is disengaged. On this account the solution of the residual ioduret of sodium in water is slightly alkaline. 100 parts of water at 58° dissolve 7·3 of this salt. It contains no water of crystallization, and is not altered by exposure to the air. When decomposed by heat, it gives out 24·45 of oxygen, a result which does not sensibly differ from the 24·43 of oxygen deduced from the proportions of iodate of potash, and from the ratio of the oxidation of potassium and sodium. I shall adopt this last number in preference, and conclude from it that iodate of soda is composed of

Oxygen	24·432
Ioduret of sodium	75·568
	100·000

On putting iodine into a solution of soda, so that the liquid was upon the point of becoming coloured, I obtained by evaporation fine crystals in six-sided prisms, with bases perpendicular to the axis, very alkaline, and deflagrating strongly upon burning coals. These crystals are very soluble, and contain a great deal of water of crystallization. As they were formed in the midst of a liquid containing hydriodate of soda, chlorine produces in them a precipitate of iodine. I consider them as a subiodate of soda. On adding soda to a neutral solution of iodate of soda, I converted the whole into crystals; but instead of being bulky, like the preceding ones, they were in small silky needles, united in tufts. They were not altered by exposure to the air, though they were very alkaline. Potash forms likewise an iodate with excess of base, and crystallizable. In general, the iodic and hydriodic acids have a great tendency to form subsalts.

The iodates of potash and soda detonate by percussion, but very feebly, when they are mixed with sulphur. It might be thought that they could be employed with advantage in the manufacture of gunpowder. But a very simple calculation will show that nitre is much preferable. 100 parts of nitre, when decomposed by heat, give 53·62 of gas, while 100 of iodate give only 22·59. Therefore, neglecting here the difference of density of azote and oxygen, and observing that oxygen in the detonation of gunpowder is chiefly employed in the production of carbonic acid gas, the volume of which is equal to its own, nitre has the advantage, every thing else being equal, of giving 2·3 times as much gas as it. It is possible, however, that the detonation of powder made with the iodate may be more rapid than that of powder made with nitre.

Iodate of Ammonia.

We can only obtain this salt by saturating with ammonia iodic acid, or the solution of chloruret of iodine. It is in small granular crystals, the shape of which I have not been able to ascertain. When thrown upon burning coals, or upon a hot body, it detonates with a hissing noise, giving out a weak violet light and vapours of iodine. I endeavoured to decompose it by heat in a glass tube, but it broke the apparatus. I collected, however, enough of the gas evolved to ascertain that it was a mixture of oxygen and azote. When we calculate its composition from the preceding data, we find it composed of

Iodic acid	100·00
Ammonia	10·94

But 100 iodic acid contain 75·80 of iodine; and if we reduce this quantity to volumes by dividing 75·80 by 8·6195, the specific gravity of iodine in vapour we find 8·794. If we divide 10·94 by 0·59669, the specific gravity of ammonia we find 17·587, which is the double of 8·794. Hence iodate of ammonia is composed in volume

Ammoniacal gas	2
Vapour of iodine	1
Oxygen gas	2·5

The two volumes of ammoniacal gas give, when they are decomposed, one volume of azote and three of hydrogen. The hydrogen requires for its saturation 1·5 oxygen. There then remains one volume of oxygen, or a bulk equal to that of the azote. On detonating iodate of ammonia, I have obtained the two gases in nearly that proportion.

Iodate of Barytes.

We obtain it very easily, either by double affinities, or by putting iodine into barytes-water. It precipitates in powder, which is obtained pure after being several times washed. This salt, while drying, concretes into lumps, and becomes mealy. Though kept for a long time in the temperature of 212°, it gives out water before being decomposed by heat; so that it seems to retain water in combination. The products of its decomposition are oxygen, iodine, and barytes, sensibly pure. This barytes dissolves very slowly in water; and as it retains no iodine, though that which is obtained by the decomposition of the nitrate combine with it, I presume that the difference proceeds from the water which the iodate contained being retained by it. Iodate of barytes is the least soluble of all the alkaline iodates. 100 parts of water dissolve only 0·16 at 212°, and 0·03 at 64·5. It is composed of

Iodic acid	100
Barytes	46·340

It does not deflagrate on burning coals, but only gives out now and then a weak light. The difference in this respect between iodate of potash and iodate of barytes depends on two causes: first, the barytes not being reduced like the potash by iodine, it gives out less oxygen than the iodate of potash; but the second, and principal cause, is, that the iodate of barytes and the product of its decomposition being infusible, only a very small portion of the iodate comes in contact with the coal, and all the rest of the salt is decomposed, without its oxygen serving to the combustion at all. The iodate of potash, on the contrary, being fusible, as likewise the ioduret of potassium, all the parts of the salt come successively in contact with the coals, and support the combustion in a powerful manner. Among the sulphates, we shall find several that detonate, if that name is to be given to those which are decomposed by fire and give out oxygen. This is the case with alum, and with sulphate of zinc. They do not support combustion, for the same reasons that iodate of barytes does not.

Iodate of strontian is obtained like the preceding salt. It is in small crystals, which viewed through a glass appear to be octahedrons. It gives out water before it is decomposed by heat, and the products which it gives out are perfectly similar to those of iodate of barytes. 100 parts of water dissolve 0·73 of it at 212°, and 0·24 at 59°.

Iodate of lime is usually pulverulent; but it may be crystallized in the hydro-chlorate or hydriodate of lime, which augment its solubility. It then assumes the form of small quadrangular prisms. 100 parts of water dissolve 0·98 of it at 212°, and 0·22 at 64°. The quantity of water which it holds in combination appears to me to amount to about three per cent. The products of its decomposition by fire are the same as those of the iodates of barytes and strontian. These three salts require a higher temperature for their decomposition than the iodate of potash.

To obtain the other iodates I employed double decompositions. Nitrate of silver gives with the iodate of potash, and even with iodic acid, a white precipitate, very soluble in ammonia. If we saturate the alkali with sulphurous acid, the precipitate appears again; but it has lost its solubility in ammonia, because it has been changed into ioduret of silver. This experiment furnishes us with the means of distinguishing in a combination hydro-chloric, hydriodic, and iodic acids, and of separating them from each other. If we treat the precipitates obtained by nitrate of silver with ammonia, those containing iodic acid, or hydro-chloric acid, will be dissolved. If we saturate the solution with sulphurous acid, and treat the precipitate again with ammonia, the chloruret of silver will be alone dissolved.

On digesting in iodic acid oxide of zinc recently precipitated and well washed, I obtained a pulverulent salt, but little soluble in water, which deflagrates on burning coals, but much more feebly than iodate of potash. We may obtain the same salt by mixing a solution of sulphate of zinc with that of a soluble iodate. No precipitate appears at first; but after some hours, small crystals are deposited, sometimes in grains perfectly spherical, which are iodate of zinc. It is necessary for success in this experiment that the sulphate should not be very much concentrated, for its viscosity would oppose the motions of the molecules, and of course the formation and separation of the iodate of zinc.

The solutions of lead, of pronitrate of mercury, of pernitrate of iron, of bismuth, and copper, give with the iodate of potash white precipitates soluble in acids. The solutions of peroxide of mercury and of manganese were not altered.

There do not exist any iodureted iodates; at least I have not been able to form them. Iodates and iodic acid do not dissolve more iodine than water.

To complete the history of the saline combinations of iodine, it remains to be determined whether, when a base acts upon this substance in water, the two salts which may be obtained exist separately in the solution, or if they are formed at the instant that any cause whatever determines their separation.

If we mix together the neutral solutions of iodate and hydriodate of potash, they do not mutually decompose each other; but if we add any acid whatever to the mixture, even carbonic acid, which is driven from all its combinations by hydriodic acid and iodic acid, iodine is precipitated, owing to the mutual decomposition of the two salts.

To render the solution of iodate and hydriodate entirely the same with that which we obtain in making iodine, potash, and water, act upon each other, and which is always alkaline, it is sufficient to add to the former the quantity of potash necessary to bring it to the same degree of alkalinity. In that case we can no longer distinguish the one from the other. It would appear, then, that the iodate and hydriodate of potash are formed at the instant that the iodine acts on the alkali in water; but that the oxygen of the iodic acid and the hydrogen of the hydriodic acid preserve a great tendency to combine together, and that it is sufficient to favour this tendency in order to cause it to take effect. Iodic and hydriodic acids, and in general all acids produced at once by the two elements of water, destroy each other when mixed together.* This is the reason why a very weak acid precipitates iodine from a mixture of iodate and hydriodate; for how weak soever its action should be, it will always decompose a small portion of each salt, as Berthollet has shown, and the acids separated producing immediately iodine which precipitates, the decomposition may continue, and make great progress, without, however, becoming complete.

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* M. Berthollet has observed that sulphurous acid and hydro-sulphuric acid may exist together when dissolved in a great deal of water. The same thing holds with the two acids of iodine, which if concentrated give a copious precipitate of iodine as soon as mixed, but do not decompose each other when diluted.

The solution of neutral iodate and hydriodate, and the solution of iodine in potash, present, however, this remarkable difference, that the first always preserves its neutral state, while the second is constantly alkaline; but if the two salts be really formed at the instant iodine is mixed with solution of potash, it would appear that we ought either to obtain a complete saturation of the alkali, or that the mixture of neutral iodate and hydriodate ought to become alkaline as soon as it is made. The reason why this is not the case is, that we cannot suppose that in a mixture of various bodies each element acts rigorously, as if it were merely mixed, and as if the mobility of its particles were perfect. We ought rather to admit, that to destroy a compound stronger forces are necessary than those that formed it. On this supposition it is easy to conceive that the alkalinity of the solution of iodine in potash, and the constant neutrality of a mixture of neutral iodate and hydriodate, may take place at the same time; and of consequence, that the two salts of which we have been speaking may be formed, and exist separately, in a solution of iodine and potash.

Hydriodic Ether.

I have paid but little attention to the action of iodine on animal and vegetable substances; yet it may produce several new combinations. MM. Colin and Gaultier have described that which iodine forms with starch; and I shall now make known an ether formed by hydriodic acid and alcohol.

I mixed together two parts in volume of absolute alcohol and coloured hydriodic acid of the specific gravity 1·700. I then distilled the mixture in a water-bath. I obtained an alcoholic liquid, perfectly neutral, colourless, and limpid, which, when mixed with water, became muddy, and let fall in small globules a liquid at first milky, but which gradually became transparent. This liquid is hydriodic ether. What remained in the retort was very dark coloured hydriodic acid. Thus in this experiment a portion of the alcohol had united with the hydriodic acid, and formed an ether, which distilled over with the rest of the alcohol, and there remained hydriodic acid very dark coloured, because it held in solution all the iodine which had coloured it at first. It was probably the action of the iodine and the water which prevented the whole hydriodic acid from combining with the alcohol.

Hydriodic ether, after having been washed two or three times with water, in which it is but very little soluble, is perfectly neutral. Its odour is strong, and, though peculiar, is analogous to that of the other ethers. After some days it acquires a red colour, which does not afterwards increase in intensity. Potash and mercury destroy this colour on the spot by uniting with the iodine to which it is owing. Its specific gravity at 72·5° is 1·9206. It boils (as determined by its tension) at 148·6°. By direct experiment, I found it to boil at 148·1°. It is not inflammable; it exhales only purple vapours when thrown upon burning coals. Potassium may be preserved in it without alteration. Potash produces no immediate alteration on it. The same is the case with nitric and sulphurous acids and chlorine. Concentric sulphuric acid renders it speedily brown. When passed through a red-hot tube, it is decomposed. I obtained an inflammable carbureted gas, very brown hydriodic acid, and a little charcoal. Besides these products, I obtained, by putting a solution of potash into the tube in which I produced the decomposition, a substance in flocks which refused to dissolve in the alkali and in acids. This substance, after being several times washed in cold water, preserved an ethereal odour, but not so strong as that of the liquid ether. In boiling water the flocks united together, and melted into a matter, which, after being cooled, resembled white wax in appearance. When put upon a burning coal, it gave out without flaming vapour of iodine in greater abundance than the hydriodic ether. It volatilizes, but much more slowly than the ether.

From these properties I consider the substance of which I have been just speaking as a peculiar ether, formed without doubt by the combination of hydriodic acid with a vegetable matter different from alcohol. I have not analyzed hydriodic ether; but on comparing it with hydro-chloric ether, which, according to Thenard, is composed of a volume of hydro-chloric gas and half a volume of pure alcoholic vapour, I consider its composition as analogous. According to this hypothesis, hydriodic ether is composed in volume of

Hydriodic gas	1
Alcoholic vapour	0·5

or in weight, of

Acid	100
Alcohol	18·55

and hydro-chloric ether, of

Acid	100
Alcohol	64·67

Supposing this composition correct, it is astonishing that hydriodic ether is not inflammable, while hydro-chloric ether possesses that property in a great degree. We cannot suppose that this difference depends on the proportions in weight of the acids to alcohol. I rather think that hydriodic ether is not inflammable, because its acid is decomposed by oxygen without producing flame, and hence the oxygen becomes too small in quantity to support the combustion of the alcohol. It would be easy to verify this conjecture by burning the ether in oxygen gas; for if it is well founded, the combustion in this gas should be attended with flame.

On calling to mind the different experiments in this memoir, we shall see that there is not one which authorizes us to consider iodine as a compound body, or as a substance containing oxygen. On the contrary, we must be struck with the resemblance which it bears in some cases to sulphur, and in others to chlorine. Like them, it forms two acids, one by combining with oxygen, the other by combining with hydrogen: and we may have remarked that the acids formed at once by the combination of chlorine, iodine, and sulphur, with the elements of water, present this remarkable property, that when the acid formed by oxygen has its elements very condensed, that formed by hydrogen has them very weakly united.

Thus sulphur takes oxygen from iodine, and iodine takes it from chlorine; but, on the other hand, chlorine takes hydrogen from iodine, and iodine from sulphur.

If we follow this analogy still further, we find that it holds likewise with respect to carbon; for sulphur deprives it of hydrogen, but yields to it oxygen. Thus it would appear that the more a body condenses oxygen, the less it condenses hydrogen.* This is, without doubt, a cause why the very oxidable metals, as iron, manganese, &c. do not dissolve in hydrogen. I say one of the causes; for if it were the only one, we could not see how mercury, silver, and gold, do not combine with hydrogen, though they have a very weak affinity for oxygen.†

The other analogies which iodine has with sulphur and chlorine are very numerous. Some iodates resemble the chlorates exactly; but most of them have a greater analogy with the sulphates. The iodurets, sulphurets, and chlorurets, in general exhibit the same phenomena with water; and the action of sulphur, iodine, and chlorine, upon the oxides, with or without water, is exactly similar. In short, all the properties of iodine may be classed between those of sulphur and chlorine. It is not necessary to remark, that though

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* From these considerations I do not hesitate to class azote with oxygen, iodine, chlorine, and sulphur. Nitric acid has a considerable resemblance to iodic acid and chloric acid by the property which it has of being easily decomposed; and because azote takes, like chlorine and iodine, two and a half times its volume of oxygen. The nitrates are decomposed by the fire, like the iodates. But we do not know any oxide from which azote disengages oxygen, from which we can conclude only that it has much less energy than this last body. Azote forms likewise with chlorine and iodine combinations which are easily decomposed, which shows that it has little affinity for them, and that it approaches them by the nature of its energy. If it does not form an acid with hydrogen, this is doubtless because in ammonia there are three volumes of hydrogen for one of azote; and in all probability, in order to produce an acid, only equal volumes are requisite. The acid combination of azote and hydrogen appears to me realized in prussic acid, which, from some experiments that I have made, and shall soon publish, I am induced to consider as an acid analogous to the combinations of chlorine, iodine, and sulphur, with hydrogen; only that its radicle is a compound of azote and carbon. Oxygenated prussic acid corresponds to chloric and iodic acids.

† See note B at the end of this memoir.

I have restricted myself to compare iodine with sulphur and chlorine, we might find analogies, though less numerous indeed, between it and phosphorus, and several other bodies; but I thought it better to compare it only with those bodies to which it bears the greatest resemblance, and among which I conceive it ought to be classed. I have been led by this to show that sulphur possesses all the general properties of chlorine, and that we ought in consequence to place it among those bodies which form acids by combining with hydrogen.

(*To be continued.*)

ARTICLE II.

Analysis of the Excrements of the Boa Constrictor. By Dr. Prout.

THE substance sent me as the excrements of the boa constrictor was solid, of a white colour inclining to yellow. Fracture earthy. When rubbed on a hard surface it left a white mark like chalk. Its feel, however, was rather more dry and harsh than that of chalk, and it was more friable than that substance. Smell faint and mawkish. Sp. gr. 1385.

I.—*A.* After making a few general experiments to ascertain its nature and composition, 50 grains, well dried and in a state of fine powder, were digested for 24 hours in dilute muriatic acid; the mixture being occasionally, during that time, gently heated and well shaken. On adding the muriatic acid no effervescence was perceived. The acid was then poured off, and in order to remove the whole of it, the substance was repeatedly well washed with distilled water, which washings were added to the acid. It was now carefully dried and weighed, and was found to have lost 4·92 grs., which, of course, were taken up by the muriatic acid, and remained in solution in it.

B. On the substance which had been left undissolved by the acid, and which had become of a bluish tinge, a solution of pure potash was poured, which, on the application of heat, completely dissolved it, forming a solution transparent and nearly colourless. To this solution, while still warm, was added muriatic acid in excess, which occasioned a copious white precipitate in the state of very minute but distinct grains, which, after having been well washed, was collected and dried. The following were its properties. It existed, as before stated, under the form of very minute grains of a beautiful bluish white pearly appearance. No smell. Nearly insipid. Very sparingly soluble in cold water; but rather more soluble in warm, and its solution faintly reddened litmus. Insoluble in alcohol. It combined with all the alkalies, earths, &c., forming compounds very little soluble in water. When nitric acid was poured upon it and heat applied, it was entirely dissolved; the solution, after some time, acquired a beautiful deep rose or crimson colour, which stained the skin and other animal matters of the same tinge. The colour, however, of the solution, after some time, vanished irrecoverably. In short, it had all the properties of *uric acid* in the greatest state of purity. Its quantity, as before stated, was 50 — 4·92 = 45·08; that is to say, 100 pts. of the excrement contained 90·16 of uric acid.

C. The dilute muriatic acid (*A*) which had acquired a yellowish colour was divided into three equal portions:—

1. To the first ammonia was added, so as just to neutralize the acid. This occasioned a very faint cloudiness. Carbonate of ammonia was now added, which produced no further precipitation.

2. To the second portion ammonia was added as before; but instead of afterwards adding carbonate of ammonia, the oxalate of ammonia was substituted, which produced a copious white precipitate.

3. The third portion of the dilute muriatic acid was carefully evaporated to dryness. The residuum, after having been accurately weighed, was exposed to a strong heat over a lamp. White dense fumes arose in great abundance, which were muriate of ammonia. After these had ceased to appear, the residuum was again weighed; and in a mean of two experiments, it was found to have lost ·9 gr. Now as $\frac{50}{3}$: ·9 :: 100 : 5·4 parts of muriate of ammonia in 100 parts which contained 1·7 of pure ammonia.

II.—*A.* From I. c. 2, it appeared that the muriatic acid held some lime in solution, but that it was prevented from being precipitated by the carbonate of ammonia (I. c. 1.) on account of the presence of animal matter, which was in a state of intimate union with it. To determine, therefore, the quantity of this lime more particularly, and also whether any other saline matters were present, 20 grs. of the excrements were burnt in a platinum crucible. The residuum was a small quantity of saline and earthy matters, which weighed 1·36 gr. On this residuum was poured a little distilled water, and heat applied. The water took up the alkaline salts, and was then poured off, and evaporated to dryness. These salts, in a mean of two experiments, weighed 1·20 gr. Acetic acid, on being poured on them, produced effervescence. After this addition, they were again dried, and digested in alcohol. This took up the acetate formed, which, on being decomposed at a red heat, left a quantity of subcarbonate of potash, which weighed 1·01 gr. Now 1·01 × 5 = 5·05 subcarbonate of potash in 100 parts, equal to about 3·45 parts of pure potash.

B. The residuum left by the alcohol was neutral, and weighed ·19 gr. It was found to consist chiefly of sulphate of potash, with a trace of a muriate, probably of soda. Now ·19 × 5 = ·95 parts of this mixture in 100 parts of the excrements.

C. Muriatic acid was now added to the residuum (II. *A.*) not taken up by the water, which dissolved the whole of it. To this solution was added pure ammonia. A precipitate fell, which was phosphate of lime. On adding carbonate of ammonia, there was a slight precipitate of carbonate of lime. After this phosphate of soda produced a very distinct crystalline precipitate, indicating the presence of magnesia. The quantities of these were too small to be weighed separately, but altogether their weight must have been ·16 gr. for 1·36 − 1·20 = ·16. Now ·16 × 5 = ·8, the quantity of these substances in 100 parts.

D. Lastly, the quantity of animal matter, independently of the uric acid, destroyed by combustion, was estimated at 2·94 per cent. For $100 - \overline{90{\cdot}16 + 1{\cdot}7 + 3{\cdot}45 + {\cdot}95 + {\cdot}8} = 2{\cdot}94$.

Hence 100 parts of these excrements contained

Uric acid	90·16
Potash	3·45
Ammonia	1·70
Sulphate of potash, with a trace of muriate of soda?	·95
Phosphate of lime Carbonate of lime Magnesia	·80
Animal matter, consisting of mucus and a little colouring matter	2·94
	100·

Observations.—I. c. 2. Oxalate of ammonia will often throw down lime when in a state of union with animal matter, together with the animal matter itself, when other tests will scarcely indicate its presence. This may be verified by almost any secretion from a mucous membrane, most of which contain lime united to an animal matter. For this interesting observation I am indebted to Mr. Wilson. The lime, therefore, in the present instance, was probably in union as an aliment with an animal matter analogous to mucus, and which was doubtless derived from the internal passages of the animal. It was insoluble in a weak alkaline solution, and hence could easily be separated by its means. The acid also, as before observed, took up a small quantity of a yellow colouring matter. The quantity of these, however, as estimated above, was probably a little too great, since a small portion of the uric acid was doubtless removed by the repeated washings, which of course caused these matters to appear more abundant than they really were.

I. c. 3. It is extremely difficult to ascertain the quantity of ammonia contained in a substance. I had before observed that this alkali was present, and the above mode occurred to me of estimating its quantity. Perhaps it is not very far from the truth, though it can hardly be relied upon as indicating the exact amount.

II.—*A.* The alkaline matter present was considered as potash, on account of the facility with which its subcarbonate and acetate attracted moisture from the air, and also from its precipitating muriate of platinum. This alkali, as well as the ammonia, I considered as combined with the uric acid, because no effervescence was produced on the addition of muriatic acid.

I obtained these excrements from Mr. Lean, jun. and on ascertaining their composition, went to see the animal said to be the boa constrictor, exhibited near Exeter Chânge, in order to learn a few particulars of its mode of feeding, &c. This animal is a young one, and, according to the keeper's statement, about 16 feet long. It is fed about once a month only, when a living rabbit, or sometimes two small ones, are exposed within its reach, which, after some time, it seizes, destroys, and swallows, in the manner peculiar to these serpents. The excrements are discharged at periods of about 8 or 10 days, in a softish state, about the consistence of stiff dough; but soon become hard and friable on exposure to the air. They are in the form of roundish scybalæ, and on their surface there is often a yellowish substance, sometimes in a crystallized state, which crystals I have found to be urate of ammonia, tinged with a little colouring matter. These are the only kind of fæces, or excrements, observed to come from the animal. I obtained a portion of them a few hours after they had been discharged, which had all the characters and properties of those above described, which I received from Mr. Lean.

As far as I know, uric acid has not been observed to be formed by this class of animals. Dr. Wollaston has found that birds produce more of this principle when fed on animal matters; and Fourcroy, Vauquelin, and Klaproth, found a large proportion of it in the excrements of a bird called guano, brought from the South Sea Islands.* It is certainly a very extraordinary thing, that nearly the whole of the food taken by this animal should be converted into this substance. May it not proceed from disease arising from the unnatural state in which it is kept?

Liquor Amnii of a Cow.

This had been taken from the uterus of an animal slaughtered in an early period of her gestation. It was of a yellowish colour, and had the appearance of very minute shining particles floating in it. Smell fragrant, something like that of new milk or butter. Taste bland and sweetish, like fresh whey. Foamed a good deal when shaken. Did not affect litmus or turmeric papers. Sp. gr. 1·013.

My attention was particularly directed to the principle found in this fluid by Vauquelin and Buniva, and called by them *amniotic acid.* I could not, however, discover the least traces of a similar principle; but it contained a very sensible quantity of the sugar of milk, which separated in crystals from it, when it had been concentrated by evaporation. It coagulated partially by heat, and some flakes fell, leaving the liquor nearly transparent and colourless. Acetic acid produced no coagulation, and this acid, moreover, prevented its coagulation by heat; hence it contained albumen. Muriate of barytes produced a very copious white precipitate.

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* I quote from Berzelius's View of the Progress and present State of Animal Chemistry. P. 103. 108.

From experiments, which it would be unnecessary to detail here, it was found that 1000 parts contained

Water	977
Albumen	2·6
Substances soluble in alcohol	16·6
Substances soluble in water, chiefly sulphate of soda? and other salts. Also sugar of milk	3·8
	1000·0

The principles soluble in alcohol were of a brown colour, and seemed to consist in part of the lactates, &c., as usual; but chiefly of a peculiar substance quite different from every other that I had examined, and which has a considerable resemblance in its sensible properties to the external brown parts of roasted veal.

My want of leisure prevented me from examining this fluid more particularly. It may not, however, be improper to observe, that the liquor amnii described by Vauquelin and Buniva differed very considerably from the above in its sensible qualities, as well as in its chemical ones. This dissimilarity probably arose from the different circumstances under which it was obtained, theirs, most likely, having been procured at the full period of gestation. These chemists, however, describe a principle obtained by them, soluble in alcohol, and having peculiar properties, which, though it appeared to resemble in some respects that above mentioned, differed from it in others. See Ann. de Chim. N° 99. Johnson's Animal Chemistry, vol. i. p. 274. Dr. Thomson's System of Chemistry, vol. v.

ARTICLE III.

On the colouring Matter, or Ink, ejected by the Cuttle Fish.

By Dr. Prout.

THE substance, of which an account is given below, was sent me in the original cyst in a perfectly dry state. The following were its properties.

It was hard and brittle. Fracture imperfectly conchoidal. When solid, of a brownish black colour, and exhibiting a slight pavonine lustre, on exposure to a strong light. In a state of powder, of a beautiful velvet black. No smell. Taste rather saltish. Sp. gr. in powder, 1·640.

A. 25 grains were digested in distilled water, and allowed to subside in it spontaneously, which took nearly a week to effect, so very slow was the process. This aqueous solution was brownish. It was divided into three equal parts, two of which were spent in ascertaining the effects of the following agents.

1, 2. *a.* Heat. This produced no apparent change.
b. Muriatic acid with heat. No apparent change.
c. Acetic acid produced a faint opacity not increased by heat. Prussiate of potash added to this solution produced no precipitate.
d. Oxymuriate of mercury produced a very faint precipitate after some time.
e. Subacetate of lead, no precipitate.
f. Nitrate of lead, slight brown flaky precipitate.
g. Infusion of galls, no precipitate.

Hence it contained no albuminous matter; but a small portion only of a principle analogous to mucus.

3. The remaining portion was evaporated to dryness in a glass capsule. It left a residuum, which, when strongly dried, weighed ·25 grs. Distilled water was poured on this residuum, which took up some neutral saline matter which proved to be a muriate, with a little sulphate, most probably of soda, though the quantity was so small that I could not pretend to determine this. These saline matters weighed about 0·18 gr. The rest was animal matter, apparently analogous, as before observed, to mucus, and probably derived from the membrane being the cyst in which it was contained. Now as $\frac{25}{3}$: ·18 :: 100 : 2·16 parts of these salts in 100 parts; and as $\frac{25}{3}$: ·7 :: 100 : ·84 parts of animal matter in ditto.

B. Diluted muriatic acid was now poured on the black matter, and digested with it for some time. To this dilute acid, which was perfectly transparent and colourless, was added pure ammonia. This occasioned no precipitate. Neutral carbonate of ammonia was now added, which produced a very copious white precipitate. This was carbonate of lime. It weighed 2·6 grains. Now 2 6 × 4 = 10·4 parts per cent.

C. To the above solution was next added phosphate of soda. This occasioned a very copious precipitate of the triple phosphate of magnesia and ammonia. It weighed 6 grains = 24 per cent. Now this contained 3·3* of magnesia, which combined with 3·7 of carbonic acid, formed 7 of carbonate of magnesia.

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* This is founded upon the supposition that the triple sulphate of magnesia and ammonia is composed of one atom of phosphate of magnesia, one atom of phosphate of ammonia, and five atoms of water. Such a supposition will constitute 100 parts of this triple phosphate, according to Dr. Wollaston's scale, at about

Phosphate of magnesia	35
Phosphate of ammonia	33
Water	32
	100

Now, according to Fourcroy, it is composed of equal weights of each, which comes very near the above.

D. The black residuum was now carefully dried and weighed, in order to constitute a check to the above. Its weight was 19·5 grains = 78 per cent., indicating a loss of 1·60 in the above-mentioned processes. This I considered as the pure colouring principle. It was of a fine full black colour, and possessed the shining appearance of powdered charcoal. It was insoluble in the muriatic and sulphuric acids, even when assisted by heat. Also in the acetic. Concentrated nitric acid acted on it readily, and with considerable energy, abundance of red fumes being emitted; and at length a partial solution, being formed of a very deep reddish brown colour. A solution of pure potash added to this solution produced no precipitate; but a solution of the subcarbonate of potash produced a slight one. A solution of caustic potash, assisted by heat, likewise effected a partial solution of this substance. Also caustic ammonia in a slighter degree. The colour of these solutions was of a deeper brown than that in nitric acid. The muriatic and sulphuric acids produced a slight precipitate when added to this alkaline solution, but not the nitric acid.

It burnt, without melting, with considerable difficulty, emitting the usual smell of burning animal matters, somewhat modified by a fishy odour. It left a very minute portion of reddish ashes, which proved to be a mixture of red oxide of iron, lime, and magnesia, the quantities in the order mentioned, that of the oxide of iron being greatest. Hence 100 parts of this substance contained

Peculiar black colouring matter	78·00
Carbonate of lime	10·40
Carbonate of magnesia	7·00
Muriate of soda? .... Sulphate of soda? .... }	2·16
Animal matter analogous to mucus	84
Loss....................................	1·60
	100·00

The carbonates of magnesia and lime, from their being so readily extracted almost entirely by the muriatic acid, appear to have been in a state of mechanical mixture only in this substance. The iron undoubtedly formed a component part of it, as it does in the colouring matter of the blood. The quantity I possessed was too small to enable me to make the investigation so complete as could be wished; though, from what has been said, it will appear to be chiefly characterized by its negative properties.

Mr. G. Kemp* has made some experiments on this substance in its recent state. He appears to have considered it as consisting of, or at least containing, albumen; but apparently without any very good reason. It appears to me, that all the effects of coagulation, precipitation by alcohol, mineral acids, metalline solutions, &c.,

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* Nich. Journal, vol. xxxiv. p. 34.

described by him, did not depend upon albumen, but upon a species of mucus, which, probably, had he tried it, would as readily have been precipitated by acetic acid and heat, as by any of the mineral acids. If albumen had really existed in it, I see no reason why I should not have met with, at least, traces of it in the aqueous solution, (A) since it is well known that albumen may be dried at a low temperature, without injuring its properties of dissolving in water, or, as far as I know, any of its properties.* I cannot say, indeed, how the specimen I obtained was dried; but it had the appearance of having been dried spontaneously by simple exposure to the air. The properties of the colouring matter, as described by Mr. Kemp, do not differ materially from those above described.

After all, however, it would be desirable to examine this substance in its recent state, as it appears to have undergone some changes in drying

This substance, from the length of time which it takes to subside in water, appears admirably contrived for the purpose of concealing the animal from his enemies, &c. A property also which, added to the permanent nature of its colour, must, as Mr. Kemp observes, render it valuable as an ink, or water colour.

ARTICLE IV.

Refutation of Mr. Walker's Claim to the Discovery of the Uses of the Cerebellum: with further Observations on Respiration. By Dr. Cross.

(To Dr. Thomson.)

SIR, *Glasgow, April* 8, 1815.

In the 27th number of your *Annals of Philosophy* there appeared a letter from one Dr. Leach, in which it is peremptorily asserted that Gall and Spurzheim have anticipated me in the discovery of the function of the cerebellum, and of the structure of the "*spinal mass of nerves.*"

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* I may observe in addition to the above, that as far as my observation extends, albumen, such as it exists in the blood, is not found as a product of secretion, either by a mucous membrane, or glandular apparatus. I am aware that many substances have been called albumen by different chemists, which, as Berzelius first showed, were not entitled to the name; as for example, the mucus of the gall bladder, which appears to possess many of its properties. But this is precipitated by acetic acid, even without heat. It cannot therefore be albumen, since it is well known that a solution of albumen in acetic acid may be boiled without coagulation, and that prussiate of potash precipitates it from this state of solution, as was first shown, I believe, by the above mentioned excellent chemist. Ammonia also precipitates albumen from its solution in acetic acid; but this has the disadvantage of re-dissolving the precipitate, if added in excess. Hence acetic acid and prussiate of potash may perhaps be considered as the best tests of albumen at present known. The phosphoric acids, also, and, I believe, most of the other vegetable acids, like the acetic, do not coagulate albumen.

In the 28th number, Mr. Alexander Walker comes forward, strikes Dr. Leach off the field by giving a flat denial to his most unfounded assertion, and thrusts in a claim for himself to the discovery. Mr. Walker, in quoting from my letter, begins at the middle of a sentence, and thus makes me appear to deduce a conclusion from most insufficient data. Moreover, he merely quotes my first conjectures on the subject without giving the smallest hint of the decisive experiments to which they led. Genuine philosophy ought to expand the breast with candour.

I never before saw Mr. Walker's speculations on the nervous system, or knew that they existed. Had I seen the third volume of the Archives before I wrote to you, I would not certainly have claimed the discovery of the quadripartition of the spinal marrow. This discovery, although quite original on my part, belongs, from priority of publication, to Mr. Walker. Mr. Walker, however, has not anticipated me with respect to the sacral termination of the spinal marrow. I am the first, so far as my reading has gone, to lift off the cauda equina, and show the marrow terminating at the sacrum in a sharp point like the quill of a porcupine. This discovery rather militates against the old doctrine that the spinal marrow is just a bundle of nerves proceeding to and from the brain, which doctrine Mr. Walker has adopted. "The spinal marrow," he asserts, "serves no other purpose than a nerve would have done in the same situation, although from its being protected by the canal of the vertebræ, and the productions of the cerebral membranes, it requires not the strong and more close investments which the nerves possess in order to protect them in their passage among moving organs." (Archives, vol. iii. p. 142.) I, on the contrary, view the cerebrum, cerebellum, and spinal marrow, down to the very point of this porcupine extremity, as one continuous organ, which may be styled the animal brain, while the cauda equina, and all the other animal nerves, are merely derivative.

I do not know what Mr. Walker means by quoting from the Archives about the cerebellum. His hypothesis regarding the cerebellum is, that it is the organ of volition; and he arrived at this same hypothesis by the following logical ratiocination. Because the situation of the cerebellum is opposite to the situation of the face, therefore the function of the cerebellum must be opposite to the function of the face; and it being an understood maxim in physiology *that sensation is just directly opposite to volition*, and as sensation resides in the face, so volition must reside in the cerebellum. This doctrine is said to be corroborated by this sapient consideration, "that as the organs of sense and the cerebellum are the *first* and the *last* portions of the nervous system, so sensation and volition are the first and the last of its functions." Although here the onus probandi lies with Mr. Walker, yet, to put this absurd and groundless hypothesis at rest, I may mention that volition ranks among the faculties of mind, whose organ is the cerebrum; and that affections of the cerebrum, while the cerebellum remains sound, produce palsy, which I humbly submit is just a loss of volition. Mr. Walker's rude indigested hypothesis regarding the cerebellum must therefore fall to the ground. There is not one word from him about the cerebellum supplying the face with nervous energy. On the contrary, he makes out a direct opposition between the face and cerebellum. Had Mr. Walker, in his loose, dashing, conjectural way, thrown out a hint that perhaps the face might derive its nervous supply from the cerebellum, yet he would have had no claim to the discovery; for a discovery is not made until some kind of proof has been adduced; but it happens unfortunately for Mr. Walker that there is not even the smallest hint, from the beginning to the end of his tract, that could at all lead in the smallest degree towards the discovery. On the contrary, Mr. Walker has kept his face right away from the true direction, and looks earnestly down through the foramen magnum after the posterior columns of the spinal marrow as far as his eye can reach. Mr. Walker's volition is just about as far from the function of the cerebellum as Dr. Gall's amativeness.

My letter to you does not announce that I intend to make these discoveries the subject of my promised work. My announced subject is physiology and physiognomy, in which there are yet vast regions of terra incognita.

Notwithstanding the grand eulogy bestowed by Mr. Walker upon Dr. Spurzheim's work, I cannot help viewing it, with the exception of a little anatomical discovery, as a most fanciful production. The alchemists, as a friend of mine lately remarked when talking on this very subject, did actually improve the science of chemistry, although they have never yet found out the philosopher's stone. But enough on this subject at present, as I intend to take a future opportunity of making a few critical remarks on the Gallian doctrine.

In order to render this letter more worthy of insertion, allow me to say a little in amplification of the theory of respiration broached in my former letter. Of the three fundamental functions, respiration, nutrition, and propagation, only two, respiration and nutrition, are immediately connected with the life of the individual. The third is prospectively concerned with the continuation of the species, and indeed is not evolved till an advanced period of life.

What becomes of the food which is swallowed? Some goes to the growth of the body in youth, some in certain constitutions to obesity; some passes off in alvine, urinary, and cutaneous excretions, in cerumen, snot, &c; but all these excretions, with the largest allowance, do not nearly balance the quantity of food devoured. When it is moreover considered that some animals do not grow at all from birth till death; that all animals during a great part of life do not grow; that many great eaters never become fat; that in a state of health the excretions are trifling, and consist more of noxious than of nutritive materials, and that vegetables which absorb such an immense quantity of sap have no alvine, urinary, or such other excretion whatever, the inquirer becomes quite disatisfied, and looks about for some more important function upon which the great quantity of chyle absorbed may be expended. Physiologists have traced the food through the alimentary tube and lacteal vessels into the subclavean vein; the chyle having got fairly into the circulation, is hurried, along with the blood and lymph returning from all parts of the body, to the lungs. All the blood, and all the chyle, and all the lymph, must perform the pulmonary circulation before they be admitted into the great systemic circulation. Now let us attend to what takes place in the lungs. According to your own calculation, as stated in the 737th page of the 5th vol. of your System of Chemistry, 3d edit. there are thrown out of the lungs by ordinary respiration in 24 hours no less than 40 000 cubic inches of carbonic acid, a quantity which contains about ¾ lb. avoirdupois of solid carbon. Whence is this carbon derived? The food is the only source from whence such a supply of carbon can be derived; while the blood is at once the grand reservoir of carbon to the lungs, and the vehicle of vitality to the body. By this conjoint view of respiration and nutrition, two mysteries are cleared up at once—the source of the carbon, and the primary purpose of the food. The life of man has often been poetically compared to the burning of a fire, taper, &c. This poetical turns out a scientific analogy. As long as there is a supply of fuel, and a free admission of air, the animal fire continues to burn; the carbon of the fuel combining with the oxygen of the atmosphere, and forming carbonic acid. Whenever the supply of fuel, or of atmospheric air, is interrupted, the fire declines; if the interruption is momentary, the fuel may rekindle; if the interruption has been too long, the fire goes out for ever. Thus we see that respiration is the great primary function for whose sake digestion was instituted; while all the excretions, like the ash-pits of a furnace, are things of merely secondary moment in the animal economy. To ensure a supply to respiration, carbon has been made the great substratum of vegetable and animal fabric—the chemical skeleton; so that when chyle is deficient, the very substance of the body is carried off to the lungs, and sacrificed on the shrine of respiration. No living organized body, from the primitive germ up to the adult stature, from the microscopic animalcule up to the whale, from the rudest lichen up to man, has ever been seen without organs of respiration. The cotyledons of vegetable seed must emit carbonic acid gas ere the radicle begins to send down its fibres into the earth; and if these cotyledons cease their respiration, and fail to become seminal leaves before the plumula rises and spreads its foliage to the heavens, the plant dies. If the pores of a fecund egg are stopped up, the hen may hatch upon it while she has heat in her body without bringing forth the chick. Nor does the chick, after it is fully formed, delay a moment to drive its bill through the shell for the free admission of air. The embryo, ere it begins to evolve, is connected to the uterus by means of a placenta as an intermediate agent between the fœtus and the maternal lungs; and whenever the function of the placenta and the fœtal circulation cease, that moment do the thorax and diaphragm commence operation; and if the placental function ceases for but a few minutes, before the mouth or nostrils get into atmospheric air, the child is irretrievably gone. That thorax and that diaphragm, having once begun, must continue their motion till death. The first sign of death is the disappearance of respiration; and the first sign of recovery from asphyxia is a renewal of respiration. It is not until God breathes into the nostrils the breath of life that man becomes a living creature; and no sooner does the breath cease, than man becomes a mere lump of organized clay.

Upon this view of respiration and nutrition, and upon the reciprocity of their functions, or rather upon the subordination of nutrition to respiration, I have formed a new theory of diseases, and more rational and successful methods of cure. To enter upon this subject would be encroaching in kind, as well as quantity, on your Journal. That purgation is a cure for melancholia and mania, I in the mean time take this opportunity of announcing; both to secure to myself the discovery, and the sooner to remove the most afflicting and the most horrible of all the sufferings of humanity. The rationale and cases shall be brought forward in detail elsewhere. I expect shortly to see many who had recommended or administered a few doses of physic to these diseases start forth as claimants of this discovery.

I remain, very respectfully, Sir,
Your most obedient servant,
JOHN CROSS.

ARTICLE V.

Experiments showing that in Hepatitis the Urine contains no Urea.

By Mr. C. B. Rose.

(To Dr. Thomson.)

SIR, *Eye, April 6, 1815.*

PERMIT me, through the medium of your *Annals*, to transmit to its chemical and medical readers the knowledge of the absence of urea from urine not being confined to the urine of diabetes only; for, while examining the urine of a girl labouring under a chronic inflammation of the liver, I could discover no trace of urea: indeed, its absence was as complete as in cases of diabetes mellitus; and I have ascertained this to be the case by a repetition of my experiments on the urine in several cases of acute as well as chronic inflammations of the liver. The urine operated upon in acute hepatitis was rather high coloured; in the chronic disease, it was pale; its odour not so urinous; its specific gravity less than that of healthy urine; and consequently left a smaller quantity of extract when evaporated.

Whether the above state of the urine is dependant on the dyspeptic stomach from hepatitis only, or on every case of dyspepsia, I have not yet satisfied myself; but I intend pursuing the subject with that view.

Unavoidably drawn by this discovery to turn my thoughts on the nature of secretion and digestion, I have dared to indulge in speculative ideas. That there subsists a great connexion between the stomach and the kidneys, not merely sympathetic, and that the state of the urine depends very much upon the state of the digestive organs, has long been observed; but that there existed this decided concatenation between the liver, stomach, and kidneys, was not, I think, before known. As this concatenation is demonstrated by the above circumstance, is it not probable that the kidneys have a similar consent of action with all the chylopoetic viscera? And is it too sweeping a conclusion to draw, from our present knowledge of secretion and digestion, if we say that it is probable in diabetes mellitus the saccharine quality of the urine is dependent on the morbid action of the stomach, and the absence of urea from the urine on a deranged state of the hepatic function? Or to suggest the probability that some one or other of the proximate elements of the urine is lost, or a new one added, by a morbid action of one of the chylopoetic organs? For instance, what change in the urine may a schirrhous pancreas produce? These inferences must not be tolerated until more data are produced; nor should I have hazarded either of them, had I not some reason to believe, from the examination of the urine of two dyspeptic patients, that the want of urea is observed in cases of hepatitis only.

Might not the want of urea in the urine of persons attacked with hydrocephalus, be a diagnostic mark between hydrocephalus *idiopathicus*, and hydrocephalus *from altered function* of the liver?

It is, I trust, the general opinion of the physiologists of the present day that the kidneys are not *merely separating but secreting organs:* and the absence of urea in hepatitis I consider as an additional fact in support of the latter opinion; for if that state of the stomach consequent on a morbid action of the hepatitic system is productive of a want of urea in the urine; and if urea has never been found in the blood (which it never has); must we not infer that its elements are the products of digestion; that they, in a peculiar state of combination, are presented to the cryptæ of the kidneys, where it is presumed the secretory power resides, and there recombined. I have communicated this fact, and my reflections on it, to you, in its unexplored state, merely with the view to excite an inquiry into, and an examination of the subject, by persons far more equal to the task than myself.

You may insert the following also, if you think it worth notice. Perhaps to those of your medical readers who are but little versed in chemical analysis, and not furnished with any chemical apparatus, and yet would be desirous of examining the urine in hepatitis, not only with the intent to prosecute the above inquiry, but also to assist in establishing the diagnosis of that disease, a brief relation of the method I have followed in my experiments, and a description of the apparatus I have invented to operate with, may not be unacceptable.

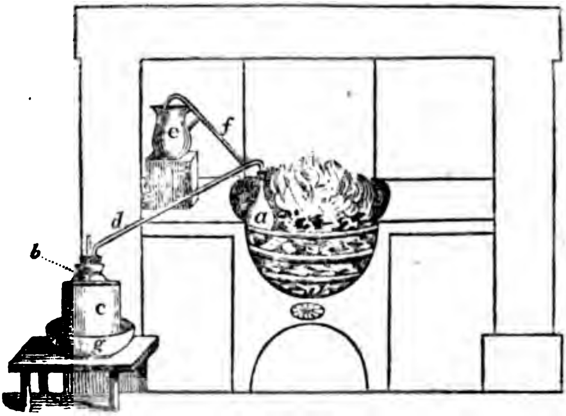
Dr. Henry, in the first number of the *Annals of Philosophy*, after having related some experiments on the urine discharged in diabetes mellitus, in which he employed the nitric acid with the extract as a test of urea, says, "There is one property, however, of this substance, originally pointed out by Fourcroy and Vauquelin, which enables us to detect urea, even when present in such minute quantities as to escape discovery by nitric acid. Amidst the great variety of animal products, this appears to be the only one which is decomposed, when in a state of solution, by the temperature of boiling water. At this low degree of heat its elements, held together by a balance of affinities which is easily disturbed, arrange themselves in a new order; ammonia and carbonic acid are generated; and carbonate of ammonia is composed, equivalent in weight to about two-thirds that of the urea. It is in the fluid, therefore, condensed during the evaporation of diabetic urine, that we are to look for traces of the existence of urea; and in this fluid I have invariably found a sufficient quantity of carbonate of ammonia to restore the colour of reddened litmus paper, and to precipitate muriate of lime." In my search for urea I have generally chosen this method by distilling the urine, and applying the tests to the condensed fluid, in preference to the more tedious process of evaporation for the extract, &c. In Dr. Henry's paper, (*Annals of Philosophy*, No. I.) and in his Elements of Experimental Chemistry, may be found information sufficient to enable any one to accomplish the examination of urine for the above purpose with the necessary precision.

Having collected the condensed fluid, I put some of it into a wine-glass, to which I add some solutio muriatis calcis; if a precipitate subside, I drop some acidum muriaticum into the liquid, which, from its greater specific gravity, passes to the bottom of the glass, comes in contact with the precipitate, and a *brisk effervescence* follows, the decisive test of the existence of urea in the urine.

Not being provided with retorts, &c. the following apparatus is what I have constructed to effect the distillation of the urine, and it answers the purpose extremely well. My retort is a Florence oil-flask; to which, by means of a perforated cork, I adapt a barometer tube, which has a small part at the end next the flask, bent to rather an acute angle, and at the other end a portion sufficiently long that it may reach nearly to the bottom of the receiver, bent to nearly a right angle, so that when the opposite ends are placed in their respectives places the tube lies in an inclined plane. By means of a leather collar fastened to the top of a jar, I fix in it a wide mouthed bottle for a receiver. The lower end of the tube passes into this, through a perforated cork, and a small short tube is passed into the receiver to admit of the exit of air. This cork should be luted into the bottle; perhaps putty is the best lute. The jar is filled with cold water as a refrigerator; but the principal agent in condensing the vapour is a glass syphon (and so efficient is it, that the condensed vapour falls guttatim into the receiver). It is placed so as to convey cold water on to the tube which connects the retort with the receiver, and this, trickling along it, is received by a large bason, in which the jar and bottle stand. The tube may be luted to the flask and bottle with a stiff dough of flour and water. If the lute is not carefully, and in sufficient quantity, applied around the tube where it passes into the cork of the receiver, the water from the syphon will wash it away, and into the receiver, making the liquid cloudy, and oblige you to repeat the process. The longer the tube is to convey the vapour, and the larger its calibre, the better it is for your purpose. The calibre of the syphon may be between a twelfth and a sixteenth of an inch in diameter. These tubes may be bent in any form, with the assistance of a lamp and blow-pipe, or a blacksmith's fire. Fearful that my description of the still which I have been in the habit of using is not sufficiently clear to enable any body to construct one by it, I have subjoined a rough sketch of it as it appears when in use. The readiness with which its materials may be had, the ease with which it may be constructed, and the convenience of such a simple apparatus to the juvenile experimentalist, are things which I hope will in some measure repay you for giving the above tedious description, and the following sketch, a place, which might have been occupied by something of more importance.

I remain, Sir, yours most respectfully,

C. B. Rose.



a, the Florence oil-flask; *b*, the bottle, as the receiver; *c*, the jar in which the bottle is fixed; *d*, the barometer tube to convey the vapour; *e*, the jug of cold water; *f*, the glass syphon; *g*, the bason in which the jar stands.

ARTICLE VI.

Reply to Mr. Phillips's Animadversions. By Mr. Hume.

(To Dr. Thomson.)

SIR,

WHEN your correspondent, Mr. Phillips, shall have finished his consultation with Klaproth's Essays he will find me ready to proceed in my reply. Mr. Phillips has practised the art of garbling most dexterously in his last letter to you, it is therefore incumbent on him, in order to satisfy your readers at least, that he turn once more to the Analytical Essays, and read and explain many passages which are more immediately connected with the subject.

If Mr. Phillips be unwilling to perform the task, or ashamed to admit the truth, I shall mention a few of the items to which I refer, as proofs that Mr. Klaproth never applied nitrate of silver as a *test* for arsenic, and that he never, on any occasion, combined it with the *oxide* of arsenic, where alone it is so eminently useful; for I never heard of a single instance where the *acid* of arsenic had been exhibited as a poison from sinister motives. But I shall not take up your time and space further than to offer the following references to Mr. Klaproth's work.

Vol. i. p. 566. "Both the solutions of the arsenical *oxide* in water showed exactly the same appearances which are exhibited by any other *aqueous* solution of arsenic." Here is an example, where Mr. Phillips must acknowledge there can be no quibbling about the author's acquaintance with the superior efficacy of silver, for none was used. Mr. Klaproth then proceeds. "By combination with lime-water, they (the two solutions) yielded *arseniate of lime:* with sulphuret of ammonia, *yellow sulphuret of arsenic* (orpiment): and the *green pigment of Scheele*, with ammoniacal oxide of copper."

If the English translation here be correct, I may remark, that arseniate of lime and the green pigment of Scheele are incompatible products from a solution of white oxide of arsenic in water; an *arseniate* of lime must require the *acid* of arsenic, and Scheele's precipitate certainly is composed with copper and the white *oxide* or arsenious acid.*

In every case where great exactness was required to detect and appreciate the quantity of arsenic, I do not perceive that Klaproth ever applied silver as a test. Thus, p. 140, vol. i. no silver test was used, although at p. 142, "a weak arsenical smell was perceived." He speaks of "a slight trace of arsenic;" and *some* of it combined with the copper, and *some* with the iron." Page 158. "The muriate of silver emitted *some* arsenical vapours." Page 160. "It is then evident that this ore consists of silver, iron, *arsenic*, and antimony." In the same place he notices "*arseniated* iron." Page 526. "A grey-yellow sublimate" and "a faint odour of

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* The English translation is inaccurate. The words used by Klaproth are arsenikalische kalkerde.—T.

arsenic was perceived;" yet this article is not included in the list of component parts. Page 548. The solution of variegated copper-ore in nitric acid "was tried by the proper *re-agents* for silver, lead, zinc, *arsenic*, &c." Here I am anxious to know what re-agent or test Mr. Klaproth took for the arsenic, for, a more suitable and altogether palpable case for exercising the silver test does not occur in either volume. Page 567. "*One* grain of sublimed arsenic" is mentioned; "the arsenic escaped in vapours;"—"the cobalt (after calcination with charcoal) was not yet entirely deprived of all portion of arsenic." In this case, I must observe, arsenic seems to have been *deposited* in place of being dissolved in the nitric acid. Indeed the whole of this essay, the analysis of the white cobalt-ore, offers many opportunities which admit of the silver test; yet it was never employed, certainly from no other cause than that the author was not aware of the value of the silver as a test, and this appears so evident through the whole of the two volumes as to admit of no contradiction.

In the second volume, page 150, the nitric acid is used to dissolve the olive copper-ore, and the *acetate of lead* is preferred to collect and separate the whole of the arsenic *acid*; and this *precedes* the next step the author takes, which Mr. Phillips, to serve his own purposes, has separated from the whole process of Mr. Klaproth, viz. merely to regenerate what the author supposed to be the native compound, the arseniate of silver, described in the first volume, page 125: and this was evidently done to prove that the arseniate of lead contained no *sulphate* of lead, and that the arsenic found in the ore is in the state of an *acid*. I need not say that such conclusion must be very uncertain in all cases where an ore has been subjected to the action of *nitric* acid. At page 153, the author states that "not only the crude ore itself, but also the precipitate obtained from its nitric solution by means of acetated lead, when reduced by the blow-pipe upon charcoal, give out a vapour, which, by its garlic smell, sufficiently shows to be arsenical:" query, the *metal* or *oxide* of arsenic? The last example which Mr. Klaproth gives us in his very excellent and most valuable essays, in which the arsenic is found, is in the pharmacolite. Here it will be seen that, after all the author's experience, including that analysis quoted by Mr. Phillips, he fixes upon the *acetate of lead* alone for detecting, separating, and ascertaining the arsenic *acid*; not a word, or the slightest allusion to a silver test; nor is there a single syllable, or the most trifling hint from Mr. Klaproth, in any part of his work, to induce an operator to prefer silver to lead as a test for white arsenic.

It is with much diffidence and hesitation that I offer any observation on the works of such a celebrated master as Klaproth. It appears, however, that on many occasions the true state in which the arsenic previously existed in the ore was not completely demonstrated by subsequent experiments; for, by employing nitric acid in almost every instance, the metal became acidified, and, consequently, its primitive and more simple form entirely changed. I have occasionally analyzed many minerals in which arsenic made a prominent ingredient, and these were first treated by the following process, which I found to be very successful as a preliminary operation. I boiled the pulverized ore in a solution of subcarbonate of potash, and when the solution was filtrated and allowed to cool, a sufficient quantity of acetic acid was added to saturate the solution. In all these examples I never discovered any sign of arsenic acid; for, on presenting a piece of dry nitrate of silver (lunar caustic) to the surface of the solution, I invariably procured a bright *yellow*, and not a red or *brick*-coloured precipitate. This method, as far as I can learn, has not been practised by any other person, I may therefore take this occasion to claim it as peculiarly my own. I have had no opportunity to analyze the red arseniate of silver, or any other arseniate, but I have little doubt that, if such a compound be submitted to the same process, the result will be equally satisfactory, and that a *brick-red* coloured precipitate would confirm my anticipation of one component part of such a mineral substance being the *acid* of arsenic.

In regard to the nitrate of barytes, that its solution is immiscible with nitric acid, and that the method employed by many chemists, before my paper was published, namely, to purify the nitric acid by adding the solution of this salt, is absurd and ineffectual, I repeat, that all I have said on that subject will defy Mr. Phillips's best efforts. Dr. Duncan, jun. has read my papers, and evidently draws a very different inference from my communication, and this Mr. Phillips may see in the "Edinburgh new Dispensatory;" it therefore remains with Mr. Phillips himself to explain what M. Bouillon Lagrange and Dr. Swediaur mean by *acide nitrique*. Before I close this letter, allow me to observe, that the word *barytic* is omitted by Mr. Phillips in the quotation from me, in one part of his last letter; and I may here add, that, to serve his own cause, he made a material and very gross omission of this kind, in his observations respecting the *compound extract of colocynth*, in what has been termed his *acute* work upon the pharmacopœia, where he confounds proof spirit with the tincture expressed from the colocynth, which must be charged with vegetable mucilage and other principles, and therefore it can no longer be considered as a *proof* spirit.

I am truly sorry, Sir, to have occupied your Journal by such an unprofitable subject, I trust, however, that the present communication will be not altogether uninteresting to your readers. The only apology I can make is founded upon the very illiberal, and, I may add, unfriendly language in the latter part of Mr. Phillips's first letter, which is evidently written with a view to injure my character. I remain, Sir, your very obedient servant,

Long Acre, April 12, 1815. Jos. Hume.

[The editor hopes that neither of the gentlemen concerned will continue this dispute any longer, as it is evidently fast sinking into personal invective.]

ARTICLE VII.

Remarks on the Phenomena of Galvanism.

In the present fluctuating state of galvanic theories, when such a variety of opinions are offered for our consideration; it may fairly be concluded that every hypothesis will be noticed in proportion to its plausibility. I shall therefore endeavour to place the subject in a different point of view from any yet published; after having made a few remarks on several opinions that have been brought forward to account for the various galvanic phenomena.

When Volta offered his simple, yet ingenious, electrical theory, to illustrate the effects of the galvanic pile, or battery, it appeared tolerably satisfactory: but as galvanic facts became more numerous, and exhibited such a novelty of character and principles, the views of this eminent philosopher were too circumscribed to embrace the great number of results that were brought to light by the rapid advance of this branch of philosophy. Although the opinions of Volta were too circumscribed to keep pace with the discoveries in galvanism, still they were too ingenious to remain unnoticed, and therefore have been remodelled by several philosophers in this country, as well as in other parts of Europe. In these alterations the ideas of Volta have been generally retained, as far as respects the electrical character of the galvanic fluid; but some writers have differed from him, by supposing that the galvanic fluid is brought into action by the chemical agency of the acid, or saline solution, used in the pile or battery, and not merely by the contact of dissimilar metals, as Volta had supposed; consequently they have been obliged to ascribe different laws to this fluid from those which were maintained by that philosopher.

In pursuing the subject of galvanism, it is extremely difficult to follow the many peculiar laws of action that this electrical view of the science enjoins, and still more difficult to reconcile all the galvanic results to those principles.

To any one well acquainted with the laws of electricity, it must appear very improbable that the negative and positive, or resinous and vitreous, state of the opposite wires of a battery, on which the most interesting phenomena are said to depend, can be obtained when they are placed in a solution that is a conducting medium: and that these phenomena, which are supposed to depend on the contrary electrical states of the wires, should become more powerful as the conducting power of this medium is increased; is a result that cannot be reconciled to any well known electrical principle. By considering the galvanic phenomena agreeably to the popular theories of electricity, we must infer that many results depend on laws the very reverse of simplicity; for according to Dr. Franklin's hypothesis, we must suppose when the battery is in action that there is a redundancy at one end, and a deficiency, or less than the natural quantity of this fluid, at the opposite end of the galvanic arrangement; and we are likewise required by the same authority to believe, that the individual particles of this fluid are violently repulsive of themselves; and yet, with this repulsion towards each other, they crowd and condense themselves at one end of the battery, although every part of this galvanic combination is a conducting medium. The other popular electrical theory admits of two distinct fluids, the particles of which are supposed to be strongly repulsive of those of their own kind; but at the same time are violently attractive of the particles of the contrary fluid.

In the application of this theory to illustrate the action of the voltaic battery, we are led to believe that the electricities of the battery are separated, that the particles of the vitreous fluid are collected at one, and the particles of the resinous fluid are collected at the opposite, end of the battery. Therefore if we follow the changes that must take place according to this theory, they will appear truly extraordinary; for these contrary fluids, which are supposed to have so great an attraction for their opposite kind, must have passed each other in their progress from one part of the battery to the other, and have collected in great quantities at the contrary ends, although the particles of each fluid thus collected are violently repulsive of themselves.

The above results, agreeably to this theory, take place in the battery while every part is a conducting medium, which, according to all the well-known laws of electricity, would counteract any such unnatural accumulation. These, as well as other complicated difficulties, must have been evident to every one who has paid attention to electrical theories; and several writers have substituted a phraseology that gives a more specious illustration to certain results, but at the same time involves the whole subject in greater obscurity; for an attempt has been made to explain some of the most interesting effects in galvanism, by what are termed "*electrical energies*," without supposing the existence or action of any distinct galvanic or electric matter.

This vague and general notion may give a plausible solution to the transfer of small bodies through a fluid medium, by supposing that the electrical character of the wires from the battery influences and gives a similar character to the particles of the fluid medium that come in contact with them, and that these portions of the medium influence or give a corresponding electrical character to their adjoining particles. That this communicating principle of the electrical energies may extend itself through the whole of the medium, and produce the transfer in question, does not appear very improbable: but this principle will not readily explain the heating and melting of steel wire when placed in the circuit of the battery; for we can scarcely imagine that the particles of a steel wire ten or twelve inches long, which are evidently held together by a stronger cohesive force than the particles of any other body in nature, can be heated red-hot and torn asunder merely by the wires from the battery being in contrary electrical states, without imparting any active matter to the wire, which must be the case if we do not admit the existence and circulation of the galvanic fluid or fluids.

Several other difficulties might be mentioned that must attend this electrical view of the science; but as the chief object of this communication is the introduction of views which appear more consistent with the general phenomena of galvanism, and not the refutation of previous opinions, it will not be necessary to dwell on any other particulars. When we impartially reflect on the many important facts offered for our consideration by the present advanced state of galvanism, and observe the great degree of ingenuity displayed in the application of various theories to account for the numerous galvanic phenomena, we must feel disappointed at the little theoretic satisfaction that has been obtained from the knowledge of so many interesting facts, and the united efforts of so much ingenious labour, and be led to infer that the chief cause of this disappointment has been the adoption of principles that are incompatible with the physical operations of Nature.

The speculations of the celebrated Franklin, perhaps, unfortunately gave too specious a facility to his electrical illustrations, as they appeared to explain in a tolerable manner all the electric facts that were known at the period he lived; but they ascribed laws of action to the electric matter that involved the subject in complete mystery; for results, that seemed to open an extensive field for inquiry, were illustrated by principles that were beyond the limits of human comprehension, and in a great measure superseded the necessity of an experimental investigation. After a view of these difficulties, our attention is naturally invited to the increasing probability that these galvanic agents have characters and qualities less mysterious than has been generally imagined, and that their principles of action are uniformly consistent with the general laws of matter. Although it is evident, from our present imperfect knowledge of galvanism, that every hypothesis must be incomplete, still there seems to be a number of well-known facts that are worthy of particular attention; and if an opinion is formed, having a judicious reference to these circumstances, it may hasten the developement of that chain of experimental results, on which a correct and complete system of galvanism will be ultimately established. By attentively considering the action of the voltaic battery, I have been induced to suppose that two galvanic fluids are generated during the process, consisting of a large portion of caloric, and two distinct and highly attenuated bases, that partake of an oxygenous and hydrogenous nature; and in this communication I shall endeavour to point out the reasons that led to this supposition, hoping they will stimulate some other person to extend the investigation agreeably to this view of the subject. In pursuing this investigation, our illustration must in a great measure depend on deductions drawn from various analogies; and as these remarks have a particular allusion to the similarity of character there is between some of the gaseous bodies and the galvanic fluids, a general view of those analogies will be found of considerable advantage throughout the most obscure and difficult part of the inquiry.

If a united stream of oxygen and hydrogen gases be lighted, and blown upon bodies that are even difficult of combustion, they will be quickly consumed by the continued action of these gases; and it is also a fact, that when the two galvanic fluids are thrown upon the same class of bodies they will produce similar effects. In the common and regular process of combustion the presence of oxygen is indispensably necessary; but when the bodies to be burnt are placed in a gas that contains no oxygen, or even in an exhausted receiver, they may be consumed by the agency of the galvanic fluids.

When a galvanic battery acts in the most powerful manner, particularly in the display of its chemical energies, there is a singular coincidence of results during its action that is worthy of notice, for a part of the water in the battery is decomposed, and at the same time two galvanic fluids appear to be liberated from the battery, whose character, in many other instances besides those just mentioned, bears a strong resemblance to the two gases obtained from the decomposition of water.

At present we may not be able to comprehend clearly the law by which these newly discovered galvanic combinations are generated; but if we follow the evident changes that water undergoes by its union with different portions of caloric, we may perhaps venture to infer that these galvanic agents are generated during the action of the battery by a principle somewhat similar.

Water in its solid form, or character, of ice, is united comparatively with a small quantity of caloric, when combined with a larger portion of this agent, it obtains the liquid state: but if united to a still greater portion, it assumes the character of steam, and often displays a mechanical force that is as singularly striking as the power of the galvanic fluids. By the application of a greater degree of heat than what is necessary for the changes already stated, the constituent parts of water become separated, and assume the state of two invisible gases, whose peculiarities approximate much nearer the character of the galvanic fluids: for the fact that oxygen and hydrogen gases mixed in proper quantities enter into combustion, when sufficiently influenced by heat or a mechanical force, presents a strong feature of those agents; as it is more than probable, from the velocity with which the galvanic fluids move, that in many instances they produce a high degree of temperature by their mechanical action upon each other, and the bodies that impede their free circulation, which gives rise to their combustible energies.

Although a variety of circumstances already mentioned appear to support the idea that the bases of the galvanic fluids are derived from the liquid in the battery, and partake of an oxygenous and hydrogenous nature; still it is extremely probable that these newly discovered compounds are formed by a very different union with caloric from that which takes place in the formation of oxygen and hydrogen gases, as we have not the least evidence that the greatest possible attenuation we could obtain by the application of heat to these gases would impart to them any electrical energies.

If we take into consideration the subtile and active nature of caloric, and compare it with the most attenuated gaseous bodies yet known, we shall see the probability that other compounds of this nature may be formed by the action of various chemical and mechanical forces, that must hold a rank between gaseous bodies and caloric; and, according to the established rules of analogy, are likely to be endowed with energies strongly resembling the powers of the galvanic fluids. The idea that a class of highly attenuated compound bodies holds a place between well-known gaseous bodies and caloric, deserves perhaps more attention from its reasonableness, than any direct evidence we at present possess; as we cannot fairly suppose that this agent, which in its active state converts the hardest and closest metals into vapour, and can by its influence change a solid mass of ice into two combustible gases, should lose this transforming power exactly at the point where it gives these bodies this gaseous character, when there is evidently such a wide difference between the comparatively dense nature of these gases and caloric.

If we admit the probable existence of these highly attenuated compounds, no results can be more reasonably expected from their action, than some of the effects we obtain by galvanism; as combinations partaking of a small portion of oxygen and hydrogen, intimately combined with a large quantity of caloric, must partake in a great degree of its active and penetrating nature, while their bases are calculated to produce many similar effects to those which are obtained by the energies of the galvanic fluids. This appears a simple mode of accounting for the production of what are termed the galvanic fluids; and the principle by which they seem to obtain their energies appears consistent with the idea that they are generated by the action of the battery.* When a galvanic combination produces a strong chemical action, these fluids are given out in great abundance, and it is not improbable that they are propelled along the conducting circuit by the high degree of elasticity which every succeeding portion obtains at the point of generation. In some experiments made to show the transfer of acid and alkaline bodies by the galvanic influence, the results obtained seem intimately connected with this part of the inquiry, as they evidently point out two opposite galvanic currents, and support the above opinions.

The experiments were made by using two small gold conical cups; in one was placed a solution of the sulphate of potash, and in the other pure water, and the liquid in the two vessels was united by moistened amianthus; when the cup in which the saline mixture was put was galvanized positively, and the other cup negatively, the solution in the positive cup was soon found to have acquired a considerable degree of acidity, while the potash of the solution was transferred to the cup that contained the pure water. But when that cup which contained the saline mixture was galvanized negatively, and the other positively, the potash was left, and the acid portion transferred to the other cup, clearly showing, when a solution which contains an acid and an alkali is decomposed by the action of the galvanic fluids, that the positive fluid transfers the alkaline, and the negative fluid the acid, part of the solution.

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* In these remarks we have supposed the galvanic fluids to be produced by a strong chemical action; but when obtained without this violent action, their character approaches much nearer to common electricity, and will be considered with greater propriety under a more general view of the subject.

This transfer of an acid substance through a fluid conducting medium, from the negative to the positive wire, appears the most conclusive fact ever published in favour of two distinct electric or galvanic fluids; as we cannot, on any rational principle, explain this transfer of matter, if we suppose the wire called the negative wire to be deficient in galvanic electricity, and supplied by a current passing from the opposite or positive wire.

The various results attending the heating and melting of steel wire when placed in the galvanic circuit also support the idea of two opposite galvanic currents. These effects most probably arise from the two opposite currents not having room to pass each other freely along the small steel wire; and their action upon each other gives birth to that combustible energy which their constitutional character is calculated to support. Although we are furnished with such strong evidence that there are two different and opposite galvanic currents in most experiments; yet it must seem rather extraordinary that these fluids should pass each other in contrary directions, when each fluid is generally supposed to have so great an attraction for the opposite kind, as in most instances to produce combustion by the violence and rapidity of their union. These remarks naturally introduce another very interesting question, viz. What evidence have we that the galvanic or electric fluids have such a strong attraction for each other? But as an inquiry into this question is not necessary for my present purpose, I shall defer it until another opportunity. In the investigation of any difficult subject, when we have no clear and positive evidence to direct our conclusions, we must apply to the aid of analogy, and take advantage of the most appropriate facts which the present state of the subject furnish for our consideration. By the rapid union of oxygen and hydrogen gases, we obtain results that resemble most of the combustible effects of galvanism; still we know the two gases show no strong signs of attraction for each other, without the application of an extra portion of heat, or a mechanical force; may we not therefore infer that it is by a similar law that the galvanic fluids unite and produce their combustible effects; for although their constitutional nature will allow them to pass each other in a complete galvanic circuit, yet when they rush upon each other from the opposite wires of a battery, which are the terminating points of a broken circuit, the mechanical action resulting from their contrary forces may induce them to unite with such rapidity as to render manifest all their combustible energies. This principle of action, and the probability that the bases of the galvanic fluids partake of an oxygenous and hydrogenous nature, will enable us to form a tolerably correct idea of the combustible effects of galvanism; but the most perplexing results attending the galvanic phenomena are said to be the invisible transfer of different bodies through various fluid media.

From the view we have taken of the subject there must be at the same time a distinct fluid quitting the end of each wire that proceeds from the battery, when they are placed in an imperfect conducting fluid medium; and to keep up the evident circulation, each fluid must endeavour to gain the wire opposite to the one it has quitted; it is therefore highly probable that the opposing forces of these contrary currents of the galvanic fluids, give rise to their powers of decomposition, rendered so manifest at the end of each conducting wire of a battery.

These general conclusions give us a new hypothetical view of the galvanic phenomena, the truth or correctness of which, will be the best ascertained by its application to explain what are termed the most perplexing results in galvanism.

In attempting to account for the invisible transfer of acid and alkaline matter through various fluid media, and the appearance of oxygen and hydrogen gases at the opposite wires of a battery, when separated by a column of water some feet in length, the correctness of this hypothesis will be put to a tolerably fair trial.

If we consider the characters we have attributed to the galvanic fluids, the invisible transfer of this acid and alkaline matter in opposite directions appears consistent with the view we have taken of the subject: for if the base of the positive fluid partakes of an oxygen nature, this fluid will probably convey to the negative wire, by the influence of affinity, the alkaline part of any saline solution which is decomposed at the positive wire, and deposit the greater portion of this transferred matter at the negative wire, when it enters that metallic part of the circuit.

We have supposed also that the base of the negative fluid may partake of an hydrogenous or alkaline nature, consequently this fluid may by the force of affinity convey the acid portion of any saline solution decomposed at the negative end of the battery, towards the positive wire; and there deposit this acid matter, when it enters the metallic part of the circuit; and this exchanging process most probably goes on, until the alkaline part of the solution is collected round the negative wire, and the acid portion of the same compound is collected round the positive wire of the battery. On this principle we may account for the appearance of oxygen and hydrogen gases at the opposite wires of a battery, though separated by several feet of water.

When water is decomposed by the galvanic action, and two distinct gases appear at the opposite wires, although separated by such a body of water, one of these gases must have been transferred in an invisible manner from one wire to the other, or the water must have been decomposed at each wire, and the constituent portions which do not appear at the point of decomposition must have become so far changed and influenced by the fluids from the battery, as to have passed with them through the water in an invisible state. This latter opinion, though rather novel, is agreeable to what has been advanced, and is strongly corroborated by the transfer of other substances, as well as those just mentioned. By keeping in view the preceding illustration of galvanic effects, it will appear that the hydrogen of the water decomposed at the positive end of the battery will be transferred by the positive fluid towards the negative wire, and there liberated: and that the oxygen of that portion of water decomposed at the negative end of the battery will be transferred by the negative fluid to the positive wire, and be there liberated; and ascend through the water in the character of oxygen and hydrogen gases.

These inferences are supported by the fact, that all the bodies collected and liberated round each wire of a battery possess such characteristic properties as are likely to be influenced by the attractive affinity of the galvanic fluids; if we admit that these fluids possess the constituent nature ascribed to them in this communication.*

The positive evidence we have that the most dense bodies can be transformed by the agency of caloric to assume so many characters, naturally suggests the idea, that a great variety of combinations may take place by its union with the constituent parts of water, which are still unknown; and no products are more likely to be among this class than those elastic compounds which, in all probability, form the galvanic fluids; as they seem a link between well known geseous bodies and caloric, by partaking of the constitutional character of the one, and the action and subtle nature of the other. Nor is it improbable but both the electric and galvanic fluids will, at some advanced period of these sciences, be considered merely as a newly discovered class of peculiar gaseous bodies, sufficiently attenuated by various degrees of caloric to give them different electrical energies.

It is not the results mentioned in this paper only that support this mode of reasoning, for the whole series of regular galvanic effects, as well as several anomalies, seem to point at something of this nature; and as opinions formed agreeably to this view of the subject will account for most of the galvanic phenomena in a simple and plausible manner, without the aid of mysterious principles, the subject assumes an highly interesting character, by the increasing probability, that the phenomena of galvanism are most intimately connected with many other important branches of natural philosophy.

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* In a small essay I lately published on Electricity, I have endeavoured to shew the probability that the electric fluids excited by the machine consist of a large quantity of caloric intimately united to a small portion of oxygen and nitrogen obtained from the atmosphere by the mechanical action of the cylinder and rubber. Perhaps if the machine was so constructed that the cylinder could be surrounded and worked alternately in different kinds of gas, the electric fluids excited under these circumstances might display a variety in their chemical action that would lead to some interesting results; and it is not extremely improbable, but that the galvanic fluids would also manifest some variety of character, if they were excited by different agents properly calculated for such a purpose.

ARTICLE VIII.

Defence of the Opinion that all Numbers have Four Imaginary Cube Roots. By James Lockhart, Esq.

(To Dr. Thomson.)

SIR,

I AM much obliged to Dr. Tiarks, and to your Correspondent N. R. D., for their attention to my late communication. The disagreement of these Gentlemen in respect of the value of the imaginary quantity gives me encouragement to hope that some doubt of the error which they suppose I have made will be excited. Dr. Tiarks affirms that the quantity is nothing but a different form of a well-known root of 64; whereas N. R. D. insists that it is a cube root of 8, and *not* of 64; and thus it would appear that the quantity is the square, and the square root of itself also. If impossible expressions, only a little complicated, universally lead to such difference of sentiment, it will be wise to abandon them altogether. Nevertheless, it now becomes me to endeavour to show that I have not made a hasty assertion, and that I was duly acquainted with the nature and construction of the quantity in question; and for this purpose I resort to the following remarks and demonstration.

In the general equation $x^3 - b\,x = c$, there are three roots, x the greater, $-t$ the middlemost, and $-v$ the least. The rule promulgated by Cardan gives all the three values, which, however, is denied by some eminent algebraists of the present day. I shall place the cube roots in their order under Cardan's binomials; and I believe that it is the first time of their being so exhibited.

$$\begin{array}{cc}
\sqrt[3]{\frac{c}{2}+\sqrt{\frac{c^2}{4}-\frac{b^3}{27}}} & \sqrt[3]{\frac{c}{2}-\sqrt{\frac{c^2}{4}-\frac{b^3}{27}}} \\
\frac{x}{2}+\sqrt{\left(\frac{x^2}{4}-\frac{b}{3}\right)} & \frac{x}{2}-\sqrt{\left(\frac{x^2}{4}-\frac{b}{3}\right)} \\
-\frac{t}{2}+\sqrt{\left(\frac{t^2}{4}-\frac{b}{3}\right)} & -\frac{t}{2}-\sqrt{\left(\frac{t^2}{4}-\frac{b}{3}\right)} \\
-\frac{v}{2}-\sqrt{\left(\frac{v^2}{4}-\frac{b}{3}\right)} & -\frac{v}{2}+\sqrt{\left(\frac{v^2}{4}-\frac{b}{3}\right)}
\end{array}$$

By means of these roots, and 28 varieties connected with them, the cube roots of all binomials may be obtained, if such roots admit of a finite expression, even when they are irrational, and without trial or assumption.

The imaginary quantity which I introduced relates only to t, and to the second cube root in the column on the left hand, which cube root is thus demonstrated to be exact :—

Let $b\,t - t^3 = c$

$$\frac{b\,t - t^3}{2} = \frac{c}{2}$$

$$\frac{b^2\,t^2}{4} - \frac{b\,t^4}{2} + \frac{t^6}{4} = \frac{c^2}{4}$$

$$\surd\left(\frac{b^2\,t^2}{4} - \frac{b\,t^4}{2} + \frac{t^6}{4}\right) = \surd\left(\frac{c^2}{4}\right)$$

subtracting $\frac{b^3}{27}$ internally to the parenthesis

$$\surd\left(\frac{b^2\,t^2}{4} - \frac{b\,t^4}{2} + \frac{t^6}{4} - \frac{b^3}{27}\right) = \surd\left(\frac{c^2}{4} - \frac{b^3}{27}\right)$$

$$\text{or}\left(t^2 - \frac{b}{3}\right) \times \surd\left(\frac{t^2}{4} - \frac{b}{3}\right) = \surd\left(\frac{c^2}{4} - \frac{b^3}{27}\right)$$

adding $\frac{b\,t - t^3}{2}$ to one side of the equation, and its equal $\frac{c}{2}$ to the other side.

$$\frac{b\,t - t^3}{2} + \left(t^2 - \frac{b}{3}\right) \times \surd\left(\frac{t^2}{4} - \frac{b}{3}\right) = \frac{c}{2} + \surd\left(\frac{c^2}{4} - \frac{b^3}{27}\right)$$

extracting the cube roots

$$-\frac{t}{2} + \surd\left(\frac{t^2}{4} - \frac{b}{3}\right) = \sqrt[3]{\frac{c}{2} + \sqrt{\frac{c^2}{4} - \frac{b^3}{27}}}$$

No other value can be used in this case for the cube root of the binomial, which the algebraist may readily prove by adapting it to an irreducible equation where there is no ambiguity in respect of the square root. Such is the equation $x^3 - 63\,x = 162$, where the binomial is $81 + \surd - 2700$, and the cube root for t is $-3 + \surd - 12$.

To obtain the imaginary quantity which is the subject of consideration, I employed the reducible equation $x^3 - 24\,x = 72$, where $x = 6$, $t = 3 + \surd - 3$, $v = 3 - \surd - 3$; and by Cardan's rule the roots of the equation are thus expressed:—

$$\sqrt[3]{36 + \surd\,784} + \sqrt[3]{36 - \surd\,784}$$

and by the previous demonstration, the cube root of the binomial on the left hand connected with t is the quantity I gave; namely,

$$-\frac{3 + \surd - 3}{2} + \surd - \left(\frac{13}{2} - \frac{3}{2}\surd - 3\right).$$

Algebraists universally give precedency in magnitude to the binomial on the left hand; and in this they follow the old masters.

It would be strange indeed to call the first binomial $\sqrt[3]{8}$, and the latter $\sqrt[3]{64}$.

The binomial on the left hand being, then, by common consent and usage, equal to $\sqrt[3]{36 + 28}$ or $\sqrt[3]{64}$, it follows that my number is a true cube root of 64, and not of 8, which your correspondent N. R. D. affirms it to be, and that I have properly, and in conformity with the practice of algebraists, taken the positive square root of 784.

I conceive, therefore, that I have now only to show that the quantity is different from the known forms of the cube roots of 64. Dr. Tiarks has divided $x^3 - 64$ by $x - 4$, and by means of the quotient he obtains $-2 \pm \sqrt{-12}$, which are the cube roots connected with the equation $x^3 - 48x = 128$, where by Cardan's rule the roots are represented by $\sqrt[3]{64 + \sqrt{4096 - 4096}} + \sqrt[3]{64 - \sqrt{4096 - 4096}}$, and where, by the roots previously exhibited depending on t and v, the cube roots become $-2 \pm \sqrt{-12}$; but these are the cube roots of binomials in their vanishing state, in which state they have functions and connexions widely different from those deduced from binomials which are not evanescent.

The means taken by Dr. Tiarks to prove my quantity to be equal to $-2 - 2\sqrt{-3}$ is by no means sufficient.

This, as well as the correctness of my assertion, may be sufficiently evidenced by the nature of vanishing fractions; and on this evidence, and not on any ambiguity of expression, I entirely rest my opinion.

If binomials are not in a vanishing state, one of the roots of the equation from which the binomials are deduced will, by a simple operation, become extinct; but all the roots will be preserved if the binomials are evanescent.

$$\text{Thus let } 3x - x^3 = 2$$
$$x - x^3 = 2 - 2x$$
$$\text{or } x - x^3 = 2 \times \overline{1 - x}$$
$$\therefore \frac{x - x^3}{1 - x} = x^2 + x = 2$$

Here the roots are preserved, because the binomials connected with the given equation vanish. But

$$\text{Let } 7x - x^3 = 6$$
$$x - x^3 = 6 - 6x$$
$$\text{or } x - x^3 = 6 \times \overline{1 - x}$$
$$\therefore \frac{x - x^3}{1 - x} = 6$$

Here the value of unity is extinct, because the binomials

$\sqrt[3]{-3 + \sqrt{-\frac{100}{27}}}$, $\sqrt[3]{-3 - \sqrt{-\frac{100}{27}}}$, do not vanish. The cube roots in respect of the root unity are $\frac{1}{2} - \sqrt{-\frac{25}{12}}$ and $\frac{1}{2} + \sqrt{-\frac{25}{12}}$; but if, under this conception, we should assimilate the sum of these roots to the root of the equation $3x - x^3 = 2$, a greater mistake, in my opinion, could not be made. In the same manner may my quantity be divided by 4, and it will be a cube root of unity, but never can it be conceived to be a root of the equation $3x - x^3 = 2$; but if Dr. Tiarks's number be so divided, it will be, together with $-\frac{1}{2} + \sqrt{-\frac{3}{4}}$, a root of the equation $x^3 - 3x = 2$. The equations $7x - x^3 = 6$ and $3x - x^3 = 2$ have a similar root unity; but it is seen that all equality is lost when they are converted into fractions, and this is precisely the case of our two numbers. It is the province of the lovers of the science to decide on the question.

I am, Sir, your obedient servant,

May 9, 1815. JAMES LOCKHART.

Another Communication on the same subject.

(To Dr. Thomson.)

SIR, May 8, 1815.

As the subject proposed by Mr. Lockhart on the algorithm of imaginary quantities is one of considerable importance in a variety of analytical investigations, you will be induced probably to admit a few remarks on the two answers published in your last number.

The first thing which appears singular is, that one of your correspondents has shown Mr. Lockhart's expression to be the cube root of 64, but under a different form to that usually given; and the other, that it is not the square root of 64, but of 8.

The fact is, that Mr. L.'s expression, $-\frac{3 + \sqrt{-3}}{2} + \sqrt{-\left(\frac{13}{2} - \frac{3}{2}\sqrt{-3}\right)}$, the same as all other quantities in which the sign of the square root enters, admits of two values; and as there is no previous condition, either of them may be employed; and the quantity will be accordingly either the $\sqrt[3]{64}$ or $\sqrt[3]{8}$. R. N. D. is therefore too positive when he says, "it is not the cube root of 64, but of 8." He is also wrong in stating that by squaring $a\sqrt{b}$ an ambiguity is introduced; for the ambiguity has place in the $\sqrt{b}$ before the operation of squaring; in fact, the only case in which there is no ambiguity is when we know the origin of the quantity whose root is to be extracted, as is shown in one of the latter numbers of Nicholson's Journal, where the object was to explain why $\sqrt[3]{\frac{1}{2} - \frac{1}{2}\sqrt{-3}} + \sqrt[3]{\frac{1}{2} + \frac{1}{2}\sqrt{-3}}$, which is known to be equal to 1·87938, or 2 sin. 70°, is not (when squared by the usual process) equal to the square of the same number. The square of $\sqrt[3]{\frac{1}{2} + \frac{1}{2}\sqrt{-3}} + \sqrt[3]{\frac{1}{2} - \frac{1}{2}\sqrt{-3}} = -\left(\sqrt[3]{\frac{1}{2} - \frac{1}{2}\sqrt{-3}} + \sqrt[3]{\frac{1}{2} + \frac{1}{2}\sqrt{-3}}\right) + 2$, which is certainly not equal to $\overline{1\cdot87938}|^{2}$.

Query the cause of this anomaly, if we admit the ambiguity to arise as stated by your Correspondent R. N. D.?

Yours, &c.

MATHEMATICUS.

ARTICLE IX.

Astronomical and Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North. Longitude West in Time $6''\frac{22}{100}$.

April 26, Emersion of Jupiter's	11h	8′	50″	Mean Time at Hackney Wick.
1st Satellite	11	8	57	Ditto at Greenwich.
May 4, Immersion of Jupiter's	10	1	49	Ditto at Hackney Wick.
3d Satellite	10	1	56	Ditto at Greenwich.
May 4, Emersion of Jupiter's	12	41	07	Ditto at Hackney Wick.
3d Satellite	12	41	14	Ditto at Greenwich.
May 12, Emersion of Jupiter's	9	26	14	Ditto at Hackney Wick.
1st Satellite	9	26	21	Ditto at Greenwich.

Magnetical Observations.

1815.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
April 18	8h	25′	24°	15′	33″	—h	—′	—°	—′	—″	6h	45′	24°	20′	08″
Ditto 19	8	35	24	15	26	1	05	24	25	58	6	40	24	18	37
Ditto 20	8	35	24	17	53	1	25	24	29	18	—	—	—	—	—
Ditto 21	8	30	24	17	56	—	—	—	—	—	6	25	24	19	31
Ditto 22	8	40	24	16	54	1	45	24	25	46	—	—	—	—	—
Ditto 23	8	35	24	16	05	1	40	24	27	06	6	25	24	19	02
Ditto 24	8	30	24	15	50	—	—	—	—	—	6	25	24	20	15
Ditto 25	8	35	24	16	04	—	—	—	—	—	—	—	—	—	—
Ditto 28	8	35	24	17	32	1	05	24	25	49	—	—	—	—	—
Ditto 29	8	20	24	19	27	—	—	—	—	—	6	35	24	22	18
Ditto 30	8	20	24	16	30	1	35	24	29	21	6	50	24	18	50

Magnetical Observations continued.

1815.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
May 1	8h	40′	24°	14′	50″	1h	55′	24°	29′	12″	6h	50′	24°	20′	22″
Ditto 2	8	30	24	20	00	1	20	24	27	24	—	—	—	—	—
Ditto 3	8	35	24	17	23	—	—	—	—	—	—	—	—	—	—
Ditto 4	8	35	24	17	02	1	55	24	22	12	6	25	24	18	24
Ditto 5	8	40	24	16	00	1	05	24	25	37	6	36	24	19	11
Ditto 6	8	40	24	14	56	1	10	24	26	46	6	40	24	18	41
Ditto 7	8	35	24	14	59	1	35	24	28	12	6	35	24	19	08
Ditto 8	8	40	24	16	56	—	—	—	—	—	—	—	—	—	—
Ditto 10	8	35	24	20	55	1	35	24	26	26	6	25	24	20	55
Ditto 11	8	25	24	14	30	—	—	—	—	—	—	—	—	—	—
Ditto 14	8	35	24	16	55	1	40	24	24	55	7	10	24	19	26
Ditto 15	8	35	24	16	05	1	25	24	28	33	—	—	—	—	—
Ditto 16	8	45	24	17	28	—	—	—	—	—	7	10	24	18	21
Ditto 17	8	30	24	17	12	1	40	24	26	58	7	10	24	20	37

		1813.			1814.			1815.		
April......	Morning	24°	00′	18″	24°	12′	53″	24°	16′	01″
	Noon	24	21	12	24	23	53	24	27	42
	Evening..........	24	15	25	24	15	30	24	17	48

April.—The variation of the weather this month has been equalled by the uncommon variation of the needle.

Rain fallen { Between noon of the 1st Apr. / Between noon of the 1st May } 1·735 inch.
Evaporation during the same period ....1·90

ARTICLE X.

ANALYSES OF BOOKS.

Transactions of the Geological Society, Volume 2d. London, William Phillips, 1814.

This volume contains 24 papers, and is accompanied by a volume of plates.

I. *On certain Products obtained in the Distillation of Wood, with some Account of Bituminous Substances, and Remarks on Coal.* By J. Macculloch, M.D. F.L.S., Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice President of the Geological Society.—

When wood is exposed to destructive distillation there comes over a thick black fluid like tar. Great quantities of this substance are obtained at the powder works from the distillation of willow and alder. It was this substance that Dr. Macculloch examined. It is very inflammable, and may be burnt like oil in a lamp. When it is washed in water, that liquid separates a considerable proportion of acetic acid, coloured by an oily matter, which has an empyreumatic smell and taste. When triturated, or boiled with carbonate of potash, it acquires a pitchy consistence; but does not seem to combine fully with the alkali. It is soluble in alcohol, ether, caustic fixed alkaline lees, acetic acid, and the mineral acids. Fat oils and fresh essential oils dissolve it imperfectly; but the drying oils and inspissated essential oils act on it more readily. Coloured oil of turpentine dissolves a good deal of it. Naphtha has scarcely any action on it. When exposed to a heat just sufficient to keep it boiling, an oil comes over, at first light coloured, but becoming darker as the process advances. If the heat of the retort be gradually increased to redness, nothing remains but a spongy charcoal. There is found in the receiver an oil and acetic acid, combined with a little ammonia. No gas is evolved in this process, if the heat be carefully managed. When a gas is formed, the oil has been exposed to too high a temperature, by letting the fire act too much on the upper part of the retort. If the heat be continued for a certain time, what remains in the retort resembles petroleum; if longer, maltha; if longer, bitumen; and if still longer, only coal remains. But these resemblances are merely external. Bitumen and our substance are different in their composition; since the first is soluble in naphta, the second insoluble. Dr. Macculloch conceives it probable that vegetables may have been converted into bitumen or jet, by the action of water, and the bitumen afterwards converted into coal by heat. He shows that heat is incapable of bituminizing wood, but that it converts jet into coal. Many other very ingenious hypotheses respecting the origin of the various species of coal and plumbago occur in this paper; but they are of so bold a nature, and so little supported by the present state of our knowledge, that I am afraid to enter upon them, least they should lead to a tedious controversy about a subject, the decision of which is at present obviously beyond the reach of our faculties. The valuable part of the paper is the chemical description of the new substance obtained by distilling vegetables, which Dr. M. proposes to distinguish by the name of *bistre*.

II. *Mineralogical Account of the Isle of Man.* By I. F. Berger, M. D. M.G.S.—The Isle of Man, the situation of which is too well known to require specification here, is rather more than 30 miles long from north to south, while its breadth varies from 15 to eight miles. The northern extremity is tolerably low for about five miles, where a range of mountains commence that proceed to the southern extremity. This group of mountains consists of three chains, separated from each other by high table lands. Three very narrow openings cross the group from east to west. The furthest north of which lies between Douglas Town and Peel Town; and the furthest south is the narrow channel between the island and Calf of Man. The first of these is highest, and the last lowest, being under the level of the sea. The highest mountain in Man is Snowfield, which is 2000 feet above the level of the sea. Dr. Berger has given the elevation of 89 mountains and spots of this island, which has enabled him to give interesting sections of the island in various directions. The Calf of Man is 206 feet high. The mean annual temperature he reckons from the heat of 31 springs, at 49·99°, or 50°, Fahrenheit.

Very little granite has been observed in Man. It occurs, however, in two or three spots; but most likely only forming beds in the clay-slate, which constitutes the central and highest part of the island. Grey-wacke covers the lower parts of this clay-slate, almost surrounds it, and constitutes the sea-coast both on the east and west side of Man. This grey-wacke sometimes contains beds of grey-wacke slate and flinty slate. It has not been observed to contain any animal remains. A floetz lime-stone containing madreporites, and some univalve shells, occupies the south-east coast; except a peninsula of old red sand-stone, which occurs likewise at Peel Town. The Calf of Man is grey-wacke. Veins containing galena occur in three different parts of the island, and always in the grey-wacke. Sulphuret of copper likewise occurs. These veins have been wrought more than once, but are at present abandoned. Dr. Berger thinks that the southern part of the island has at some former period subsided. Hence he accounts for the gradual declivity, the dip, and the narrow valleys. The population is reckoned at 28,000; but Mr. Curwen thinks it does not exceed 23,000.

III. *On the Granite Tors of Cornwall.* By Dr. Macculloch.—The Logging Rock, and Cheese Wring in Cornwall, and the Vixen Tor in Dartmoor, exhibiting curious configurations of granite rocks, are described and figured; and Dr. Macculloch shows very clearly that they have resulted from the natural effects of the disintegration of the rock, and that they have not, as some supposed, been formed artificially. He terminates the paper with some speculations on the origin of granite, and rather inclines to its igneous origin. Into these speculations I think it unnecessary to enter; because I conceive them to be entirely beyond the reach of the human faculties. Supposing a man to spend his life in guessing how the Almighty brought the earth into its present state, and supposing him even to guess right, (no very probable supposition) I ask by what means he could prove his conjecture? The only conclusive evidence in such a case is historical evidence, and where could it be found? *Geology* does not consist in speculating about the origin or creation of the earth, it consists simply in determining the *number*, *nature*, and *position* of all the rocks which constitute the surface of the earth; and till it is confined to this, it can never become a useful nor correct science. If you can demonstrate by petrifactions the epochs of the formation of rocks, or the changes they must have undergone, do so; but never reason or speculate from the mere love of hypothesis.

I may just observe for the consideration of Dr. M., that nothing is more common than to find sand-stone having exactly the structure of his Cornish granite, I mean decaying ultimately into balls. As examples, I may mention a sand-stone in the Isle of Skye, a sand-stone in Fifeshire, and another in Dumfries-shire. It follows, therefore, that if the Doctor's reasoning about the igneous origin of granite be accurate, he must admit likewise the igneous origin of sand-stone.

IV. *Notes on the Mineralogy of the Neighbourhood of St. David's Pembrokeshire.* By John Kidd, M. D. Prof. Chem. in the University of Oxford, M. G. S.—It would appear from Dr. Kidd's description, that the fundamental rock of this district is a syenite, which seems in some cases to pass into granite, in others to porphyry; but, unfortunately, the descriptions are not sufficiently minute to make us sure of these transitions. Beds or rocks of these three different species, however, occur here. Over the syenite, and, in general, covering the flat country, lie beds of clay-slate, which vary a good deal in their appearance. This slate is often black, has little lustre, and contains quartz veins running through it; hence it is probably a transition slate. Dr. Kidd calls it *grey-wacke*; but that term never can be applied correctly to a slate. Indeed it is quite obvious from Dr. Kidd's observations, that the term *grey-wacke* has no fixed meaning in his mind: yet it is as well defined a rock as any other, and any mineralogist in five minutes might make himself so well acquainted with its characters, as afterwards to run no risk of confounding it with any other. The following definition may be of some use.

"Grey-wacke of Werner is a conglomerated rock with a basis of clay-slate. It contains portions or fragments of clay-slate, flinty-slate, quartz; and sometimes grains of felspar and scales of mica."

If Dr. Kidd dislikes the term *grey-wacke,* he may substitute *transition sand-stone.* The word grey-wacke was invented by the miners at Freyberg, where the rock abounds. Werner adopted it from them, and drew up an accurate description of the rock. Very good specimens of it are to be found in the Pentlands and Lamermuirs, near Edinburgh. About 100 yards east of the fourth milestone from Truro in Cornwall, there is a quarry of excellent grey-wacke, which is employed in mending the road. Many other British localities might be given; but from either of these two it would be easy to bring up a sufficient number of specimens to London, to make every mineralogist familiar with the appearance of this rock. I conceive that the structure of the country described in this paper by Dr. Kidd is similar to that of the

Crifle in Galloway, of which an account will be found in the *Annals of Philosophy*, vol. iii. p. 465.

V. *An Account of the Brine Springs at Droitwich.* By Leonard Horner, F.R.S. M.G.S.—Salt has been made at Droitwich in Worcester for above a thousand years. The structure of the country, as far as it has been ascertained, is this. Uppermost a bed of sand-stone, usually red, but sometimes greenish, and then containing veins of gypsum. Mr. Aikin and Mr. Horner consider it as old red sand-stone. Under this lies a bed of gypsum about 150 feet thick: below this a river of brine 22 inches deep. Lastly, there lies a bed of rock salt which has never been penetrated. If the sand-stone be old red sand-stone, the position of the gypsum and rock salt is uncommon, as these beds have hitherto been observed only above the red sand-stone and never below it. There are four pits at present used at Droitwich, and the quantity of brine which flows out is much greater than can be consumed. The quantity of salt manufactured annually is about 16,000 tons. This is chiefly consumed in England, and pays a duty of 320,000*l.* The present market price of the salt is 31*l.* per ton, 30*l.* of which is duty.

The specific gravity of the brines of the different pits is as follows:—

Walker's pit	1·20611
Walwyn's pit	1·20383
Romney's pit	1·20015
Stuckey's pit	1·18467
Farley's pit	1·17471

The last pit is not at present in use, and is probably somewhat mixed with surface water. None of them are perfectly saturated; for water saturated with salt at 55 is of the specific gravity 1·21. Walker's pit ought to contain, by my experiments, 28·88 per cent. of salt; or three ounces of it hold in solution 470 grains of salt. Mr. Horner, however, obtained by evaporation only 431·86 grains; but it is well known to chemists, that the whole of the salt dissolved in water cannot be recovered by evaporation. However carefully the evaporation be conducted, a portion of the salt always makes its escape. The quantity of salt obtained by evaporation from four cubic inches of each of the pits, by Mr. Horner, was as follows:—

Walker's pit	317·14 grains.
Walwyn's pit	313·40
Romney's pit	311·00
Stuckey's pit	283·50
Farley's pit	266·34

The salt was dried in the temperature of 180°. The constituents of this salt Mr. Horner found as follows:—

Common salt	96·48
Sulphate of lime	1·63
Sulphate of soda	1·82
Muriate of magnesia	0·07
	100·00

I doubt whether Mr. Horner's criterion of the absence of sulphate of magnesia be sufficient. After the removal of the lime he added ammonia without obtaining any precipitate. I think it probable, that a small quantity of sulphate of magnesia, held in solution in a complicated saline liquid, would not precipitate with ammonia, in consequence of the solubility of the triple salt formed. But I have not tried the experiment.

VI. *On the Veins of Cornwall.* By William Philips, M. G. S.—The ore in Cornwall occurs commonly in veins which have an east and west direction, and are called *lodes*. These are traversed by north and south veins called *cross courses*; and sometimes by north-east and south-west veins, called *contres*. The relative antiquity of these veins appears in general to be in the order in which they have been named. The lodes are the most important; they contain tin and copper ore. The tin ore is commonly towards the upper part, and the copper towards the lower part of the lode. No one of these lodes has been traced further than two miles; yet there can scarcely be a doubt that they traverse the whole county, and even pass through Devonshire, till they lose themselves in the channel. No vein has been followed to its greatest depth. Their most common breadth is from one to three feet. They are very rarely 30 feet wide. When a cross course cuts a lode it generally changes its direction, or *heaves* it, as the miners term it. The contres do the same thing to both the others. The cross courses are usually filled with quartz; though sometimes likewise they contain ore.

There is a low range of hilly country that traverses Cornwall just on the south of the principal veins. This range, as far as I observed, was granite. On the north and south side this granite is covered by clay-slate, which goes to an unknown depth. The veins usually traverse the clay-slate, and when they are followed far enough, they are found likewise entering the granite, which spreads itself below the clay-slate. Horn-stone porphyry likewise passes through the clay-slate, but whether in beds or veins has not been determined. It is called *elvan* by the miners. The top of a lode is usually called *gossan*, being filled with a yellow ochrey matter to which that name is given; and lodes have received the following names, according to the prevalent substance with which they are filled:—*Gossany*, when abounding in gossan: *sparry*, when abounding in quartz, or fluor spar, both of which are called spar, in Cornwall. The latter commonly green spar or sugar spar: *mundicky*, when abounding in iron pyrites: *peachy*, when abounding in chlorite: *fleuckany*, when filled chiefly with a kind of clay:

scovany, when composed of a mixture of quartz and chlorite; not very hard: *calpy*, when composed chiefly of hard quartz, coloured by a little chlorite: *pryany*, when the ore does not occur in a compact state; but mixed with the constituents of the lode: *grauany*, when composed chiefly of granite.

VII. *On the Fresh Water Formations in the Isle of Wight, with some Observations on the Strata over the Chalk in the south-east Part of England.* By Thomas Webster, M. G. S.—One of the most remarkable additions which geognosy has of late years received, is an accurate description of the beds which cover the chalk in the neighbourhood of Paris. These beds abound in petrifactions; by a careful examination of which it has been ascertained that some of these beds have been formed at the bottom of the ocean, while others, placed above them, have been formed at the bottom of a fresh-water lake; and these alternations are repeated more than once: so that the sea appears to have twice covered the environs of Paris, to have twice retreated while its place was supplied by a fresh-water lake. Immediately over the chalk lie the following beds: 1. plastic clay; 2. coarse lime-stone and sand-stone; 3. siliceous lime-stone, which have been formed at the bottom of the sea. Next comes a bed of gypsum, marl, &c. containing only bones of land animals and fresh-water shells, and therefore formed at the bottom of a fresh-water lake. It constitutes the lower fresh-water formation. Then come beds of marl, containing only sea shells, and therefore of marine formation. It constitutes the upper marine formation. The sixth bed consists of sand and sand-stone, without shells; the seventh a sand-stone containing sea shells; the eighth, the buhr or mill-stone formation, without shells, and argillaceous sand. Then comes the upper fresh-water formation, comprehending marls and buhrs, with fresh-water shells. This last formation is covered by alluvial soil. For a particular description of these formations we are indebted to Brogniart and Cuvier.

Though the south-east of England bears this striking resemblance to the north of France, where the Paris basin is situated, that its basis is chalk, which is covered in various places by different beds, yet Mr. Webster was the first person who pointed out a similarity in the formations in certain parts of the south-east of England and those which fill up the Paris basin. The two places where the resemblance is greatest are the Isle of Wight basin and the London basin. In the Isle of Wight Mr. Webster traced an alternation of sea and fresh-water formations similar to those near Paris, and characterized by the very same fossils. The beds, indeed, are not exactly the same in both, though there is a certain degree of resemblance between them. These different formations can be distinctly seen at Headen Hill, upon the north side of Alumbay, not far from the Needles, on the west coast of the Isle of Wight. Sir Henry Englefield first discovered a range of chalk-hills running east and west through the middle of the Isle of Wight. These hills terminate on the south side of Alum Bay, and on the north side of the chalk there occur a great number of perpendicular beds of clay and sand, often containing abundance of loose pebbles. Mr. Webster conceives it impossible that these beds could have been formed in the position which they now occupy. He supposes that they were at first placed horizontally over the chalk, and that they were thrown into their present position by some unknown convulsion. Immediately to the north of these beds is Headen Hill, composed of beds nearly horizontal. These beds Mr. Webster considers as formerly lying over the perpendicular beds of Alum Bay, before they acquired their perpendicularity.

The perpendicular beds at Alum Bay are analogous to the lower marine formation in the Paris basin; for all the fossils which they contain, though different from those in the chalk, are of marine origin. The lower fresh-water formation is distinctly seen near the bottom of Headen Hill. It consists of a series of beds of sandy, calcareous, and argillaceous marls, sometimes mixed with brown coal. The thickness of these beds is 63 feet. It contains so many fresh-water shells, and so regularly deposited, that we cannot suppose them to have been carried by rivers into the sea. Besides, in that case we should find a mixture of sea shells, which do not occur in this formation. This is covered by the upper marine formation. It consists of clay and marl, is about 90 feet thick, and contains a prodigious quantity of sea shells. This is covered by the upper fresh-water formation, about 70 feet thick, and consisting of alternate beds of sand, lime-stone, and clay. It contains abundance of fresh-water shells, without any admixture of sea-shells. This formation is covered by alluvial soil.

Mr. Webster conceives the Isle of Wight basin to have been formerly filled with these formations. It consisted of the northern half of the Isle of Wight, extended as far west as Dorchester, and as far east as Shoreham, being bounded on the north by the hills which constitute the South Downs.

The London basin is of much greater extent, but not so well defined. Its southern boundary is marked by Deal, Canterbury, Milton, Chatham, Gravesend, Purfleet, from which it runs south-westerly to Leatherhead and Guildford, and then bending northerly it proceeds as far west as Hungerford. Maidenhead, Eton, St. Alban's, mark part of its northern boundary, and it appears to take in the whole counties of Essex, Suffolk, and Norfolk. The bottom of this basin is chalk. Over the chalk lies a bed of sand, or in some places of plastic clay. Over this lies the London clay, well known for the great number of marine remains which it contains. This clay is mostly covered by a bed of gravel of various thickness, obviously alluvial. Mr. Webster considers the London clay as analogous to the upper marine formation in the Paris basin. The formations formerly placed over the London clay he considers as having been removed by some convulsion.

VIII. *Remarks on the Vitrified Forts of Scotland.* By Dr.

Macculloch.—These forts have been observed in different parts of the north of Scotland and in Galloway. Nothing is known relative to the time of their erection, or the people by whom they were built. Two opinions have been advanced: that they were vitrified on purpose, and that they were vitrified by accident. Dr. Macculloch shows, by an examination of the stones of which they are composed, that the latter opinion cannot be correct. He examined two of these forts: Dun Mac Sniochain, in Argyleshire, and Craig Phadric, in Inverness-shire. In both the walls consist chiefly of primitive rocks. These have been mixed with a considerable quantity of a kind of amygdaloid, which easily fuses, and by the fusion, more or less complete, of this material, the vitrification has been brought about. The amygdaloid differs considerably in its nature at the two forts. At Amworth, in Galloway, there is no amygdaloid; the rocks are grey-wacke and grey-wacke-slate. Some portions of this grey-wacke are fusible, from a mixture of calcareous spar. Accordingly this fort is only vitrified in a few detached spots.

IX. *On the Sublimation of Silica.* By Dr. Macculloch.—He had exposed a mixture of the oxides of tin and lead in a crucible covered by another, for some hours, in a furnace at a heat conjectured to be between 130° and 140° Wedgewood. On cooling, the top of the uppermost crucible contained some small filamentous crystals, white and brilliant, crossing each other in all directions. They were found to be crystals of pure silica. Dr. M. was not able to repeat this experiment with success.

X. *Observations on the Specimens of Hippurites from Sicily, presented to the Geological Society by the Hon. Henry Grey Bennet.* By James Parkinson, M.G. S.—These specimens are so perfect as to enable Mr. Parkinson to determine some things respecting the original structure of the shells. The supposed operculum mentioned by Picot la Peyrouse could not be distinguished. Mr. Parkinson thinks that the hippurites, orthoceratites, ammonites, &c. had the means of elevating themselves to the surface of the sea, like the nautilus; and some things which he observed in these specimens give countenance to this idea.

(*To be continued.*)

ARTICLE XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, the 27th of April, a paper by Mr. Seppings was read, containing further particulars respecting his improved mode of ship-building. As, immediately after his last paper a paper was read to the Royal Society, containing theoretical objections against a mode of building ships to which in his opinion theory was not competent, it occurred to him that it would be satisfactory to the Society to lay before them the results which have been obtained by an investigation into the ships built according to his mode. He stated the report of Admiral Durham respecting the Tremendous. She was the best sailing ship in his fleet. Three years afterwards she was examined by professional ship-builders, and found not to have suffered any deterioration. Other similar reports were mentioned. Dr. Young's allegation, that the oblique braces were imitated from the French, is, according to Mr. Seppings, inaccurate. The French had indeed introduced some pieces of oblique timber, and they were to be seen in a ship which we took from them; but they had not been attended with any benefit, and were abandoned as useless by the French themselves. Mr. Seppings's mode is quite new, and the benefit of it sufficiently obvious.

On Thursday, the 4th of May, a paper by Sir Humphry Davy was read, on the action of acids on hyper-oxymuriate of potash. In consequence of the discovery of a new acid by Gay-Lussac by treating hyper-oxymuriate of barytes with sulphur, Sir H. Davy was induced to examine more carefully than had hitherto been done the action of acids on the hyper-oxymuriate of potash. When sulphuric acid is poured upon this salt in a wine-glass, very little effervescence takes place, but the acid gradually acquires an orange colour, and a dense yellow vapour of a peculiar and not disagreeable smell floats on the surface. These phenomena led the author to believe that the substance extricated from the salt is held in solution by the acid. After various unsuccessful attempts to obtain this substance in a separate state, he at last succeeded by the following method. About 60 grains of the salt are triturated with a little sulphuric acid, just sufficient to convert them into a very solid paste. This is put into a retort, which is heated by means of hot water. The water must never be allowed to become boiling hot, for fear of explosion. The heat drives off the new gas, which may be received over mercury. This new gas has a much more intense colour than euchlorine. It does not act on mercury. Water absorbs more of it than of euchlorine. Its taste is astringent. It destroys vegetable blues without reddening. When phosphorus is introduced into it, an explosion takes place. When heat is applied, the gas explodes with more violence, and producing more light, than euchlorine. When thus exploded, two measures of it are converted into nearly three measures, which consist of a mixture of one measure chlorine and two measures oxygen. Hence it is composed of one atom chlorine and four atoms oxygen. It is not unlikely that euchlorine is a simple mixture of three measures of chlorine and two measures of this new gas; but the point cannot be determined till it be known whether Dutch foil will burn in such a mixture as it does in euchlorine. This experiment the author could not try, because at Rome, where he then was, he could procure no Dutch foil fit for his purpose. The same gas is disengaged from hyper-oxymuriate of potash by nitric acid, and with still greater facility; but it is always mixed with one-fifth of its bulk of oxygen gas. Sir H. Davy conceives that the chloric acid of Gay-Lussac contains hydrogen, and owes its acid properties to the presence of this principle.

At the same meeting of the Society, a paper by Dr. Philips was read, giving an account of the remains of a fœtus found within the abdomen of a child. The child was aged two years and a half, and had a swelling in the belly, which had been considered as dropsical, and aperient medicines had been administered. The swelling did not diminish; and the child at last was obliged to be kept always in a recumbent posture. Dr. Philips, on examining the abdomen, found a circumscribed swelling on the left side, which at first he considered as an enlarged spleen, but afterwards laid aside that opinion, without being able to form any one in the least satisfactory. The child died on the fourth day after having been seen by Dr. Philips. On opening the abdomen, the intestines were all found in a sound state, except the liver, which was indurated. The tumor consisted of a large mass of matter not connected particularly with any of the intestines, weighing eight or ten pounds, and inclosed in a very vascular bag. On cutting into it, some serous liquid oozed out. The dissection, from the situation of the medical men, was necessarily hurried and imperfect; but unequivocal traces of a fœtus were found, particularly the bones of the tibia and talus, and some others, which were found adhering together, and covered with muscle.

On Thursday, the 11th of May, a paper by Mr. Porrett, jun. was read, containing experiments to determine the composition of prussiate of mercury and of the prussic acid with the application of the atomic theory to both, and to the constitution of ferrureted and sulphureted chiazic acids. He dissolved 40 grains of prussiate of mercury in water, and decomposed it by hydro-sulphuret of potash. He obtained 37·2 grains of black sulphuret of mercury. To determine the quantity of prussic acid present in this salt, he dissolved ten grains of it in water, and mixed an hydrogureted sulphuret with the solution, which he had previously ascertained to have the property of converting prussic acid into sulphureted chyazic acid. This acid was then thrown down by means of two parts of sulphate of iron and three parts sulphate of copper; and the sulphureted chyazate of copper being dried and weighed, he was able, from his previous analysis of this salt, and of sulphureted chyazic acid, to determine the quantity of prussic acid which it contained. The result of the analysis was, that prussiate of mercury is composed of

Prussic acid	13·2
Red oxide of mercury	86·8
	100·0

To analyze prussic acid he employed the methods pointed out by Thenard and Gay-Lussac and Berzelius; but he simplified them considerably. He used prussiate of mercury, and mixed it with a quantity of red oxide of mercury, constituting a certain multiple of the red oxide contained in the salt. He found by trials that it was necessary to mix the salt with five times the quantity of red oxide which it contained, in order to obtain a complete decomposition. The two substances were intimately mixed together by trituration, and then introduced in three separate portions into a glass tube close at one end, about eight inches long, and of the size of a goose-quill. Each portion was separated from the succeeding one, by a small quantity of green glass. Another tube of the same size, open at both ends, and previously filled with mercury, was firmly tied to the retort tube by means of a piece of caoutchouc. Its extremity was then introduced into a glass cylinder filled with mercury, and placed as near as possible in a horizontal position on the mercurial trough. The mixture was then successively burnt by means of a spirit lamp, and, when cool, the whole gaseous contents were thrown into the glass jar. The azote evolved was just equal in bulk to the prussic acid decomposed, the carbonic acid formed amounted to twice the bulk of the azote. From these data Mr. Porrett concluded that prussic acid is composed of

Azote	40·7
Carbon	24·8
Hydrogen	34·5
	100·0

Or of

Azote	1 atoms
Carbon	2
Hydrogen	8
	11

Prussiate of mercury is composed of one integrant particle of prussic acid and one integrant particle of red oxide of mercury. Sulphureted chyazic acid is a compound of 1 atom sulphur + 4 integrant particles of chyazic acid. Ferrureted chyazic acid is a compound of 4 atoms black oxide of iron + 1 atom prussic acid.

LINNÆAN SOCIETY.

On Tuesday, May 2, a paper by George Montague, Esq. on the ardea nigra, or black stork, was read. This bird was shot in England.

On Wednesday, the 24th of May, the anniversary of the Society, the following office-bearers were elected for the ensuing year:—

President—Sir James Edward Smith, M.D.
Treasurer—Thomas Marsham, Esq.
Secretary—Alex. Macleay, Esq.
Under Secretary—Mr. Richard Taylor.

There were retained of the old Council:—

The President,
The Lord Bishop of Carlisle.
Aylmer Bourke Lambert, Esq.
William Elford Leach, M.D.
Alex. Macleay, Esq.
Thomas Marsham, Esq.
William George Maton, M.D.
Daniel Moore, Esq. F.R.S.
Joseph Sabine, Esq. F.R.S.
Thomas Smith, Esq.

The five following Fellows were elected into the Council:—

Thomas Marquis of Bath,
William Kent, Esq.
Rev. Thomas Rackett.
Thomas Thomson, M.D. F.R.S.
John Walker, Esq.

Since last anniversary the Society has lost nine Fellows and five Foreign Members by death; and eleven new Fellows have been elected into the Society.

GEOLOGICAL SOCIETY.

April 21.—A communication from Thomas Hare, Esq. entitled Observations on Basalt, with eight illustrative drawings, was read. In the opinion of the author of this paper, basalt is a crystallized substance, formed by deposition from an aqueous solution; its real form is spheroidal, and the columns which it usually presents result from those spheroids being heaped one on another, and from the lateral compression to which each heap is subjected by contiguous and surrounding heaps.

May 5.—A paper by the Rev. W. Buckland, M.G.S. entitled a Description of an insulated group of Rocks of Slate and Greenstone, situated on the east side of Appleby, between Melmerby and Murton, in Cumberland, was read.

The group of rocks here described runs nearly N. and S., and consists principally of slate and green-stone, the slate lying for the most part on the east of the green-stone. The order of the superposition of these two rocks appears to be very indeterminate; sometimes they abut abruptly against each other; sometimes the slate is uppermost, but most generally the green-stone. A few thin beds of blackish transition lime-stone occur in the slate; and in some places the slate is intersected by dykes of compact flesh-red felspar with scales of mica. In another place a more perfectly characterized granite makes its appearance surrounded by green-stone; but whether this is a dyke, or a projecting mass of the subjacent rock, it is not easy to ascertain.

On the east side of the range of rocks above described may be seen a conglomerate (being one of the forms of the old red sand-stone) resting immediately on the slate, and itself serving as the basis for the great lime-stone deposit which crops out in Cross Fell and forms a parallel range to the slate and green-stone. On the western side, however, of this latter range, a very different series of rocks displays itself. First occur strata of lime-stone, either by themselves or with their seams of coal interposed, elevated at a high angle approaching to vertical, and much broken. Then comes in a sand-stone in nearly horizontal beds, extremely different from the conglomerate already mentioned, and, in the opinion of the author of this paper, forming a part of the great deposit which overspreads the vale of Carlisle, a large part of Cheshire, and the vale of York, and in which are found the great quarries of gypsum and the beds of rock salt, a deposition more recent than the magnesian lime-stone which is incumbent on the upper strata of the principal English coal-fields.

ROYAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1814.

I. *Physical Department.* By M. le Chevalier Cuvier, Perpetual Secretary.

Chemistry.

One of the most curious substances lately discovered is *iodine*, a substance long concealed in kelp, which by heat is converted into a beautiful violet vapour, and which, acting on other bodies in a manner analogous to chlorine, has given new force to the notions originating from the nature of sulphureted hydrogen and from that of chlorine—notions tending to introduce into the theory of chemistry that important modification that oxygen is not the only principle capable of producing *acidity*.

In fact, Berthollet had shown about 30 years ago that sulphureted hydrogen, though it contains no oxygen, possesses all the properties of acids; and the German chemists had dwelt very much upon this fact in their disputes with the French chemists. MM. Thenard and Gay-Lussac made experiments in 1809 showing that it is impossible to extract oxygen from what was called oxymuriatic acid; and that, if it contains oxygen, we must suppose, that in all cases when it is converted into common muriatic acid, water is formed, which unites, and cannot be separated from the acid produced, or at least that the elements of the water enter into the acid as constituent parts; while on the other hand, by regarding oxymuriatic acid as a simple substance which forms muriatic acid by combining with hydrogen, these suppositions are rendered unnecessary. But though our two chemists stated these two hypotheses, they adhered to the former, which appeared more analogous to what takes place in the greater number of acidifications.

Sir H. Davy, who had been led to the same conclusions, made his decision with greater boldness. He adopted the latter hypothesis, and gave to oxymuriatic acid the new name *chlorine*, from which he derived that of the other acids into which it enters. One of these (*muriatic*), in which it is combined with hydrogen, has been called *hydro-chloric acid*; the other, in which it is combined with oxygen, *chloric acid*.

The experiments on the acid, called *fluoric acid*, led soon after to the notion that it was likewise composed of hydrogen and of a simple body of a peculiar nature, distinguished by the name of *fluorine*. This opinion was first entertained by M. Ampere, lately named a member of the section of geometry.

Thus the property of acidifying hydrogen, or of becoming acid by its means, was admitted in three substances; namely, sulphur, chlorine, and fluorine. Iodine has presented itself as a fourth body.

In our analysis of the labours of last year, we stated that iodine was discovered by M. Courtois. This skilful manufacturer appears to have discovered it about the end of 1811; but he mentioned it only to his friend M. Clement, who did not make it known to the public till towards the end of 1813. But this delay was soon compensated. In a few days M. Gay-Lussac and Sir H. Davy determined its principal properties, pointed out its analogy to chlorine, and described the two acids which it forms, like chlorine, with hydrogen and oxygen. Davy presented this analogy as a new argument in favour of the theory which he had embraced.

Since that period iodine has been examined with that interest to which it is entitled. M. Colin has examined its combinations with mercury and ammonia, and ascertained that it forms *iodic acid* (a combination of iodine and oxygen) whenever it is treated with those oxides in which the oxygen is feebly combined. He explained the generation of the fulminating powder of iodine, first discovered by M. Courtois. Ammoniacal gas is absorbed by iodine, and forms with it a viscid liquid, which changes its nature when put into water. The hydrogen of a part of the ammonia forms with a part of the iodine hydriodic acid, which combines with the rest of the alkali, and the azote of that first portion of ammonia forms with the other part of the iodine the fulminating powder.

The same M. Colin has laboured with M. Gauthier Claubry to determine the way in which iodine acts on organic bodies. These two young chemists have ascertained that those substances in which the oxygen and hydrogen are in the same proportions as in water merely mix with iodine: that those in which there is a greater proportion of oxygen combine with it intimately; but that neither of these bodies is altered, unless a heat be employed capable of decomposing them. On the other hand, those substances in which hydrogen abounds convert iodine into hydriodic acid. The same thing happens to the first class of bodies when they are sufficiently heated to let go their hydrogen. These experiments presented some curious appearances. A mixture of starch and iodine, when triturated, assumes a red, blue, or black colour, according as the iodine is more abundant, &c.

But our associate Gay-Lussac is the person who has made the most extensive and careful set of experiments on iodine. His paper has been printed in the Annales de Chimie. He examines iodine itself, its combinations, and those of its two acids with different bodies. These, according to the established rules of nomenclature, are denominated *iodurets*, *iodates*, and *hydriodates*. He treats likewise of chlorine, and makes some new remarks on its combinations, all of which had not been correctly appreciated. Then considering prussic acid as essentially formed of azote, hydrogen, and carbon, he concludes that azote ought likewise to be added to the list of substances capable of producing acids without oxygen. This leads him to consider acidity and alkalinity as properties belonging to certain bodies and certain combinations, without any necessary relation to their composition, as far as can be discovered, and which of consequence makes it approach to the ideas of Winterl and some German chemists. This memoir is full of delicate investigations and ingenious hints, of which it is not possible to give an account, but which will not fail to give a new spring to the most profound and most important department of chemistry.

Our respectable associate M. Sage, who, notwithstanding his age and infirmities, always takes a lively interest in new chemical facts, has likewise made experiments on iodine, and on kelp, from which it is obtained. He has observed the alteration produced by iodine in the silver vessels in which it is heated. Kelp furnished him, by naked distillation, products analogous to those of animals; and by macerating them in weak nitric acid, he obtained a cartilaginous net, similar to that left by bones and by madrepores when deprived of their earthy parts. M. Sage concludes, from these two facts, that the fuci are polypi.

The same chemist has presented likewise a notice on the advantages of reducing galena by the fire. He affirms that by this method much more lead is obtained than by the ordinary way.

M. Theodore de Saussure, correspondent, who in 1807 had read to the Class a memoir on the composition of alcohol and of sulphuric acid, of which we gave an account at the time, and from which it resulted that ether contains more carbon and hydrogen than alcohol, has last year resumed this important object of investigation, and making use of methods at once more simple and more rigorous, has obtained a more exact result. By passing these two liquids through a red-hot porcelain tube, he converted them into water and a gas, the analysis of which presented no difficulty. By this method he ascertained that alcohol and ether contain each an identical proportion of carbon and hydrogen, and in the same ratio that they are in olefiant gas, but combined with different proportions of water reduced to its elements.

In alcohol the elements of water form a third of the whole, and in ether they form the fifth part; so that the action of sulphuric acid upon alcohol to produce ether seems to consist only in depriving it of a portion of its water; and the same acid, when applied in greater quantity, produces olefiant gas, by removing the whole of the water.

The analytical results of M. de Saussure agree with those obtained by the late Count Rumford, respecting the quantity of heat produced by combustion of alcohol and ether.

One of the great difficulties in the analysis of organic bodies consists in this, that chemistry possesses but a small number of reactives capable of separating the immediate principles without destroying them. M. Chevreul, Assistant Chemist in the Museum of Natural History, has endeavoured to render them more useful, by employing very different degrees of heat, and thus varying their solvent power.

For this purpose he has contrived a machine, which he calls a *distillatory digester*, consisting in a Papin's digester, shut by a valve attached to a spring. The force of this spring, which is altered at pleasure, determines the degree of heat which the liquid must receive in order to escape. The produce of each degree is successively collected by means of a tube passing into a receiver. The solid matter under examination is retained in the digester by means of a diaphragm, by which it may be pressed, and all the remaining liquid forced out of it.

M. Chevreul has made experiments on cork by this method. He subjected it 20 times to the action of water, and 50 times to that of alcohol. Having thus separated various substances, there remained a cellular body, which he calls *suberin*, and which, when treated by nitric acid, is converted into *suberic acid*. Among the substances thus extracted from cork, there is one which he considers as new, and which he calls *cerin*, because it possesses several of the properties of wax.

The same chemist has applied his method to amber, and ascertained that succinic acid exists in it in a perfect state.

He has likewise continued his researches on saponification, of which we gave an analysis last year. By comparing fat in its natural state with that which has been saponified, he has concluded that the new properties of the last do not proceed from the separation or addition of any constituent, but from a new mode of combination, occasioned by the action of the alkali, which gives to fat an analogy with the acids independent of all oxygenation.

M. Pelletier, the son of our deceased associate, has examined the colouring matters from sandal wood and *alcanette* (*anchusa tinctoria*), hitherto considered as mere resins. The first, besides possessing most of the properties of resins, is soluble in acetic acid, even when very weak, and then acts upon gelatine like astringent bodies, and forms oxalic acid when acted on by nitric acid. It possesses, besides, some other characters, which seem to raise it to the rank of a peculiar vegetable principle. The colouring matter of alcanette dissolves in ether, alcohol, and expressed oils. When treated with nitric acid, it gives oxalic acid and bitter principle. Alkalies and water make it undergo various changes of colour. These united properties, in the opinion of M. Pelletier, entitle it likewise to be considered as a peculiar vegetable principle.

We have seen formerly that crude platina, as extracted from the mine, contains several foreign substances, and among others, four peculiar metals, which have been lately described. Last year we described the methods employed by M. Vauquelin to separate *palladium* and *rhodium* (two of these metals form a solution of platinum in nitro-muriatic acid); and to obtain them in a state of purity. We mentioned, likewise, that M. Laugier having perceived that this solution contains a notable quantity of a third metal, remarkable for its volatility, on which account it received the name of *osmium*, had pointed out a method of collecting it.

A black powder, which does not dissolve in nitro-muriatic acid, remained to be examined. It forms the residuum after the solution of crude platinum. It is composed chiefly of *osmium*, and of a fourth metal, to which, on account of the various and lively colours of its solution, the name of *iridium* has been given.

These two metals are united in that powder with chromium, iron, titanium, silica, and even with a little alumina. The difficulty was to separate them completely from this mixture, and to obtain them in a state of purity.

Vauquelin has succeeded in this, but by laborious and complicated processes.

Simple washing divides this powder into two parts; one, finer and more brilliant, contains more iridium and osmium, and scarcely any chromium; the other, browner and coarser, contains less of the first two metals and more of the others. As this last portion is the most difficult to analyze, we shall confine ourselves to it.

M. Vauquelin triturates it in the first place with twice its weight of nitrate of potash. The oxygen of the acid oxidates the iridium and osmium, which combine with the disengaged potash. The application of heat disengages a great part of the acid and the osmium, which are received in lime-water. The residue, diluted and saturated with nitric acid, gives a precipitate of iridium, titanium, iron, alumina, and a little oxide of chromium, and there remains a liquid composed of potash united to chromic acid and to osmium. This last metal is separated by adding nitric acid and distilling, receiving the osmium in a flask surrounded by ice. A little muriatic acid is poured into the water containing the osmium. A plate of zinc is then introduced, which precipitates the osmium. To obtain it quite pure it is washed with water acidulated with a little sulphuric acid.

The chromium is next to be separated. For this purpose the liquid is evaporated, the residue dissolved in water, and filtered, to separate the silica that may be present. Pronitrate of mercury is then poured in, which occasions a precipitate of chromate of mercury, which, being dried and heated, gives the green oxide of chromium. There remains for examination the first precipitate of iridium, titanium, iron, chromium, and alumina. There is likewise a little osmium, which is removed by digesting in muriatic acid, distilling, and precipitating by zinc as before. If there remain portions undissolved, they must be triturated with nitre, as at first; and we observe that the oftener this operation is repeated the more blue do the muriatic acid solutions become, because they contain less and less iron and titanium, which, as more easily dissolved, are first acted upon by the acid, and leave behind them a greater proportion of iridium.

Now iridium has this property: when in that state of oxidation that it forms red solutions in acids, it is only precipitated by sal-ammoniac, and that in the state of a triple salt. It is therefore brought to this state by boiling its muriatic solution with nitric acid. The liquid is neutralized by ammonia. By boiling, the iron and titanium are thrown down. The iridium is then precipitated by sal-ammoniac; and the triple salt obtained, when exposed to a red heat, leaves iridium in a state of purity.

This metal, so difficult to separate from the singular alloy which concealed it from all eyes, possesses remarkable properties. Its colour and lustre are very similar to those of platinum. It is more difficultly fusible. It is insoluble in the simple acids, difficultly soluble in nitro-muriatic acid; but potash and nitre oxidize it, and combine with it into black powder, which gives a blue-coloured solution. With boiling nitro-muriatic acid, it forms a red solution. Its blue solutions themselves become red when boiled; but both the blue and the red solutions are discoloured by sulphate of iron, sulphureted hydrogen, iron, zinc, and tin. Oxymuriatic acid causes them to resume their colour. It is iridium which gives a red colour to the last precipitates of the triple salt of platinum, while the first precipitates, into which it does not enter, are yellow.

The properties of osmium are not so easily determined, in consequence of the ease with which it is oxidated and volatilized. Its oxide is white, and very caustic. It exhales an unsupportable odour. It is flexible and fusible, like wax; and as soon as it touches an animal matter, it blackens it. Its solution in water becomes blue by nutgalls, &c.

M. Mongez, member of the Class of Ancient Literature, has read a memoir on the bronze of the ancients, in which he shows, from experiments made by M. Darcet, that it is not by immersion in cold water that bronze becomes hard, as is the case with steel; but that it acquires its hardness by being heated red-hot, and then allowed to cool slowly in the air. M. Darcet has taken advantage of this property to make symbals, instruments hitherto made only in

Turkey, and, it is pretended, by a single workman in Constantinople, who possesses the secret.

The falls of stones from the atmosphere, since the reality of the phenomenon has been constated, are observed so often, that by and by the most astonishing thing will be the long incredulity entertained respecting them. A remarkable fall took place this year in the department of the Lot-et-Garonne. It happened on the 5th of September, and, as usual, in fine weather, with a strong explosion and a whitish cloud. The number of stones was considerable; one of them was said to weigh 18 pounds. They were scattered over a surface of about a league radius. Their external characters and composition are absolutely the same as in other stones of the same origin, only their fracture has tints a little more marbled than common. Excellent reports by two good observers at Agen, MM. de Saint-Amans and Lamouroux, have made us fully acquainted with the details of the phenomenon.

M. le Comte Berthollet has presented to the Class, on the part of Mr. Tennant, one of the stones that fell last year in Ireland, and which resemble all the others, excepting that they contain a little more iron.

It is known, and we have had repeated occasions to mention it, that the stone called arragonite furnished the strongest objection that could be produced against the employment of crystallization in the classification of minerals; because chemists had found no difference between its composition and that of calcareous spar, though the crystalline forms be essentially distinct. This objection is now removed. M. Stromeyer, Professor of Chemistry at Gottingen, has discovered the constant presence of three per cent. of strontian in arragonite, while none exists in calcareous spar. M. Laugier, Professor in the Museum of Natural History, has repeated this analysis, and obtained the same result. It remains to be explained how the addition of so small a quantity of a constituent can change so completely the form of the primitive molecule of a mineral.

(To be continued.)

ARTICLE XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *River Missouri.*

The river Missouri, which was navigated in 1805 and 1806 by Captains Louis and Clarke from its junction with the Mississippi to its source, runs a course east and south of above 3000 miles. It rises in a very elevated group of mountains situated between north latitude 44° and 45°, and about west longitude 112°. The height of these mountains is unknown; but as their summits are perpetually covered with snow, we are sure that it at least exceeds 8000 feet. It runs in a northerly direction for nearly three degrees of latitude; then easterly, with still a northerly inclination, for six degrees of latitude; then nearly south; afterwards south-east; and lastly, nearly east, over a space occupying nine degrees of latitude and 13 degrees of longitude. Its size is fully as great above a thousand miles before it joins the Mississippi as at the junction, yet a great number of large rivers join it in the interval. This shows us the great evaporation to which it is subjected. It joins the Mississippi nearly in north latitude 39°, west longitude 90°, from Greenwich. The Mississippi, after this junction, flows for 10° of latitude south, a course, including the windings, certainly not so little as 2000 miles; so that the whole course of the Missouri, from its source to the ocean, exceeds 5000 miles. This is a length, of course, that will not easily be paralleled any where else; and almost the whole of this immense river is navigable. What is still more important, a great part of its banks consist of fertile plains; and from the observations of Lewis and Clarke, it would appear that a coal country occupies about a thousand miles of these plains. What a country is this likely one day to become!

II. *An Analogy respecting Volcanoes.*

The phenomena of volcanoes are some of those that have hitherto baffled the sagacity of philosophers. Hence I conceive that every analogy which has any tendency to throw light upon their origin, however insignificant in itself, ought not to be neglected. This induces me to venture to state the following one. Some time ago I got a salt from Mr. Trimmer, which is sublimed during the burning of London bricks. Mr. Trimmer informed me that the sand which is mixed with the clay in the bricks is brought from below Woolwich, and is therefore in all probability impregnated with common salt. This will account for the acid in the sal-ammoniac. I was at a loss to account for the decomposition of the common salt; but a correspondent from Bristol (*Annals of Philosophy*, v. 157,) suggests that sulphate of ammonia is generally formed during the combustion of coal. This must no doubt be the case, as pyrites is almost a constant companion of pit coal. Here, then, we have during the burning of bricks containing common salt by common pit coal the sublimation of sal-ammoniac. Now it is well known that sal-ammoniac is sublimed from Mount Vesuvius. Hence is it not likely that the fuel which supports Vesuvius is coal, and that sea water has access to it? This supposition has been made long ago; but the preceding analogy seems to me to strengthen it. Would it not be proper to ascertain whether sal-ammoniac be sublimed from all volcanoes? and if not, whether there be any thing peculiar in the position and geology of those that yield this salt? If all volcanoes yield sal-ammoniac, I should be inclined to suspect that the quantity of coal contained in the bowels of the earth is much greater, and that it goes much deeper, than has been hitherto suspected.

III. *Arragonite.*

It may be worth while to state, that Stromeyer's discovery of the presence of strontian in arragonite has been confirmed more than six months ago at Paris by the experiments of Vogel, Laugier, and Vauquelin, all of whom succeeded in obtaining crystals of nitrate of strontian from arragonite.

IV. *Whether Cast-Iron expands on congealing.*

In answer to the queries and doubts of my Correspondent N. N. on this subject, I have to observe that Reaumur made a set of experiments on the melted metals, and found that three of them expand in the act of congealing; namely, cast-iron, bismuth, and antimony; while all the rest contract. If you throw a piece (of tolerable size) of gold or silver into these metals in fusion, it immediately sinks to the bottom; but a piece of iron will swim on the surface of the melted mass. When melted iron congeals, the surface is not concave, as it would be if it contracted, but convex. The allowance alluded to by N. N. for the shrinkage is in consequence of the diminution of bulk before congealing, partly from contraction, and partly from leakage. If the iron had congealed before the addition was made, it would be in vain to make it, as the addition would not unite with the old portion.

V. *On the Production of Complimentary Colours by the successive Reflection of Polarized Light from Gold and Silver.*

Dr. Brewster has discovered that if a pencil of polarized light undergoes more than two reflections between two polished plates, either of *gold* or *silver*, it consists of two portions, A and B, of coloured light, the colours being complimentary to each other. The portion A is polarized in the plane of reflection, and B in a plane perpendicular to it. The colours of A and B vary after every two reflections. The 2d and 3d, the 6th and 7th, the 10th and 11th, &c. reflected images are *blue*, or composed of the most refrangible rays; while the 4th and 5th, the 8th and 9th, the 12th and 13th, &c. are *red*, or composed of the least refrangible rays. The angle of incidence upon the plates should be between 80° and 90°, and the plane of reflection inclined 45° to the plane of the primitive polarization. In order to perceive the colours, the reflected pencil must be analyzed by a prism of Iceland spar. This singular property is not possessed by mercury, lead, steel, nor any other metal which has been tried; but all these metals possess another property different from that of the gold and silver. These experiments have been repeated and verified by M. Biot, of the Institute of France.

VI. *On the Multiplication of Images, and the Colours which accompany them, in some Specimens of Iceland Spar.*

Professor Robison, Martin, Brougham, and Malus, have successively examined the multiplication of images, and the beautiful colours which accompany them, in some specimens of Iceland spar. All these philosophers ascribe the phenomena to *fissures* within the crystal, and almost all of them explain the multiplication of the images by internal reflections. Dr. Brewster has discovered the true cause of all these appearances, and can communicate the faculty of producing them to any piece of Iceland spar, or any double refracting crystal. What was supposed to be an accidental fissure, he has shown to be an *interrupting stratum*, either of the same or of a different substance, *crystallized in a different manner from the rest of the mineral*, and producing the complimentary colours by depolarizing two of the images; and he has found that this stratum is always perpendicular to the shorter diagonal of one of the rhomboidal faces. The artificial rhomboids which have been constructed upon this principle imitate, in the most perfect manner, all the phenomena which appear in the real crystal.

VII. *Optical Properties of Iodine.*

In attempting to obtain a measure of the refractive power of iodine by ascertaining the angle at which it polarizes light, Dr. Brewster found that it possessed the property of polarizing the light which it reflected in two opposite planes, a property which is peculiar to metallic bodies.

VIII. *Queries respecting the Visibility of the Stars in the Day-time.*

(To Dr. Thomson.)

SIR,

Having for some time past made a considerable number of observations on the celestial bodies in the day-time, the following conclusions, amongst many others, have been established, from a great number of observations and experiments, viz. that in every instance an increase of the magnifying power of the telescope has the principal effect in rendering a star easily perceptible; that the diminution of the aperture of the object glass produces a very slight effect, in some cases none at all, and that when the aperture is contracted beyond a certain limit it produces a hurtful effect, and even prevents the object from being distinguished. These conclusions, I have reason to believe, coincide with the deductions of Mr. Short and others who have made similar observations; so that it may be considered as a fact sufficiently established, that magnifying power is requisite for distinguishing a star in the day-time, and that the more the magnifying power is increased, the more splendid and brilliant the star appears.

Query: What is the cause why the magnifying power of telescopes produces this effect?

In regard to the planets, a probable solution might be given from the consideration that the telescope, by enlarging the angle of vision, augments the apparent size of the object, thus presenting a larger surface to the eye, which is partly the reason why the moon is visible in the day-time. But this reasoning will not apply to the fixed stars; since no telescope is found to augment their apparent size, so as to make them resemble planetary disks. I am aware that it may be said, that the telescope excludes almost all the light except what comes from the object, and thereby prevents the impression made by its light from being effaced; and that by increasing the magnifying power the ground upon which it is seen becomes darker, forming a greater contrast to the light of the star. That the first of these considerations, however, is not sufficient to account for the effect, I am pretty much convinced by the following experiment. I have frequently directed a long tube, blackened in the inside, and furnished with proper apertures to exclude the extraneous light, to a star, about sun-set, when it was easily seen with a very small degree of magnifying power, but could never perceive it through the tube, though I was perfectly certain it was pointed directly to the star. This experiment has uniformly failed of success in detecting a star till it became visible to the naked eye. That the darkness of the ground on which a star is seen is not, of itself, sufficient to account for the effect produced by magnifying power, is also evident from the following circumstance, viz. that by diminishing the aperture of the object glass we may produce as dark a ground as we please; but this contraction of aperture will not produce the effect of rendering a star visible if a small power be applied; nay, by diminishing the aperture beyond a certain limit, we prevent a star from being seen, which would otherwise be quite perceptible, I am therefore induced to conclude that some additional reasons must be assigned why magnifying power produces this effect. To ascertain these reasons is the object of this query.

For similar reasons, I should wish to be informed if the fact is *established beyond all doubt* that the stars are visible in the day-time from a deep well or pit. This fact is generally taken for granted, both by philosophers and by the vulgar, and has been asserted by many respectable writers, both ancient and modern; but none of those whose works I have perused assert that they themselves, or any of their learned friends, have witnessed this phenomenon. Otto Guericke, without questioning the fact, attempts to account for it in the following manner: "It is," says he, "because the light which proceeds from the stars is not overpowered by the rays of the sun, which are lost in the number of reflections which they must undergo in the pit, so that they can never reach the eye of a spectator at the bottom of it." But for the same reason they ought to be visible through a long tube, where the rays of light are prevented from striking the inside, by the interposition of proper apertures, and more especially when such a tube is accurately directed to a particular star, which is contrary to many experiments I have made to ascertain this fact. Have any of your numerous philosophical readers, or their friends, seen the stars from a deep pit in the day-time? Are colliers, miners, or subterraneous surveyors, who are frequently in favourable situations for this purpose, known to have occasionally observed them? If so, are small stars distinguishable in such situations? or is it only when a star of the first or second magnitude happens to pass near the zenith that such a phenomenon is perceived?

Is it a fact, as has been related by some authors, that the celebrated astronomer Tycho Brahe had an observatory in a deep pit or dungeon, where he frequently sat and contemplated the stars in the day-time, as reflected from mirrors which were placed around him in different positions for this purpose?

An answer to any of the above queries, if they be not deemed too unimportant for discussion in the *Annals of Philosophy*, will much oblige, Sir, yours, &c. T. DICK.

Methven, near Perth, April, 1815.

IX. *On the Explanation of the Fluctionary Calculus.*

(To Dr. Thomson.)

SIR,

The following ideas owe their origin to the valuable article of Professor Christison in your last number. Their merit is certainly only of the ordinary kind, yet their publication may perhaps do some good.

To facilitate the conception of the generation of fluxional quantities, I conceive that if the line A D (see the Professor's figure) be considered as a cylinder, on which is rolled a sheet of paper, divided into the two parallelograms, A F, C E, the former being coloured black, and the latter red; then, when the paper is unrolled, it will be easy for the student to comprehend the generation of the rectangles, and also their constant ratio to each other, which (Euclid i. 6,) is as A E to E D, or, in the Professor's example, as 5 to 1.

Again, if A C D E, in fig. 2, represent a piece of paper forming a parallelogram, as A D, and a triangle, as A B C, having a series of equidistant lines, as N M L, H G F, &c. &c. drawn parallel to C B D, and on which the respective proportions of C B to B D, of N M to M L, of H G to G F, &c. &c. are written, then if C D represent a cylinder on which the paper is rolled, commencing at C D, it is manifest when the paper is unrolled that A E will be the part first visible, and as it continues to be unfolded, the generation of the parallelogram and triangle, and also the ratio of their rates of increase, by the numbers on the parallel lines, at any instant or position, will be shown in the easiest and most familiar manner.

This simple contrivance will, I conceive, illustrate completely the Professor's idea, as the generation of the quantities can be easier comprehended by this mode than by supposing them to be produced by the motion of a line. I perfectly agree with your learned Correspondent, that the first principles of this science, and indeed somewhat more, may be attained by very young persons; and it is singular that a simple and elementary treatise adapted to their comprehension has not yet been published. An author perhaps could not be better employed than in elucidating the principles of those sciences which, from their great and important applications, are so intimately connected with the most profound and interesting departments of human knowledge.

I am not, however, a friend to the introduction of motion into the conception of fluxional quantities, as I conceive it to be foreign to the inquiry. I would rather consider the differential calculus as an extension of the science of algebra.

Your humble servant,

Plymouth, May 5, 1815. George Harvey.

X. *Milky Juice of Plants.*

Dr. John has lately examined the milky juice of several European plants. I conceive that a short account of the results which he obtained will be acceptable to the readers of the *Annals of Philosophy*.

1. *Leontodon Taraxicum* (*Dentylion*).

The juice of this plant is liquid, and milk-white. It has a sweetish and very bitter taste, with a certain degree of sharpness. Its smell is similar to that of the fresh plant. When exposed to the air, it speedily coagulates, and acquires a violet-brown colour. The coagulated sap being boiled in water, communicates to that liquid a brown colour and a bitter taste, and the property of reddening litmus paper. The undissolved portion is white and elastic, and similar to caoutchouc. It becomes brown by exposure to the air. Alcohol acts but feebly on it, dissolving only a little resin. The caoutchouc burns with a brilliant flame. The watery solution was precipitated by oxalic acid, nitrate of silver, nitrate of mercury, and nitrate of barytes. Hence the constituents of this plant are—

Water.
Caoutchouc.
Bitter extractive.
A sweet substance?
A trace of resin.
A trace of gum?
An acid.
Muriate, phosphate, and sulphate of lime, and of an alkali.

2. *Lactuca Sativa* (*Lettuce*).

This plant yields but little milky juice. According to John, its properties are the same as those of the leontodon taraxicum, and its constituents the same. He was not able to determine whether it contained an uncombined acid. From the known soporific powers of the juice of lettuce, there is reason to believe that it contains a substance similar to that which exists in common opium. It would be worth while to examine whether the juice of the leontodon taraxicum contains the same principle.

3. *Ficus Carica* (*Fig Tree*).

The twigs and footstalks of this tree yield a milky juice, which speedily coagulates in the air, and becomes a transparent mass, having the consistence and lustre of wax. The fresh sap has a bitterish and astringent taste, but produces no change upon the solutions of iron. Water dissolves a small portion of the coagulated sap. Alcohol at a boiling temperature dissolves about a third of it; but the resinous substance thus dissolved precipitates again as the liquid cools. The undissolved portion possesses the properties of caoutchouc. Hence the constituents of this sap are as follows:—

Caoutchouc.
Resin, soluble only in boiling alcohol.
A trace of extractive, soluble in water.
Salts.

4. *Platinus Occidentalis* (*Plane-Tree*).

The bark and wood of these trees when young contain a milky juice. This juice, according to John, contains the following constituents:—

Water.
Resin, soluble in boiling alcohol only.
Caoutchouc.
A very small quantity of gummy matter.
Phosphoric acid.
Salts.

XI. *Account of a dreadful Accident at Heaton Main Colliery near Newcastle.**

This colliery is situated in the bed of coal called the high main. It is of a considerable depth, about 110 fathoms, and the shaft is situated at the lower extremity of the mine. The shaft is divided by boarding all the way down, so that the same opening served for the *up* and *down* cast shaft. The seam towards the rise had been formerly worked as a colliery, under the name of Heaton Banks, by shafts distinct from the present working, which shafts, when the colliery was given up, were covered over with boards and earth. In the course of time these old workings had become filled with water; and the managers of the present colliery being well aware of the danger attending so large an accumulation of water, the workings were proceeded in with the utmost caution.

The mine was very much subject to what the colliers call the *creep*, which is a gradual filling up of the horizontal passages. It had been customary for some time past to bore in various directions upon the lines the men were working, in order to ascertain whether any body of water lay concealed in the adjacent cavities. This precaution was about to be put in practice at nine o'clock on Wednesday the 3d of May; but before that time had arrived, (between three and four o'clock in the morning,) a dreadful rush of water came through the roof in the north-west part of the colliery, and continued to flow with such rapidity, that only 20 men and boys were enabled to make their escape. In a very short time, the water closed up the lower mouth of the shaft: and that night it rose to the height of 24 fathoms. Some faint hopes being entertained that the men below would retire to the higher parts of the workings, which were said to be above the level of the water in the shaft, every exertion was used to open a communication with them by the old workings. Considerable difficulties, however, presented themselves. The rubbish which covered and choaked up the mouths of two old shafts, when deprived of the support of the water, fell in, dragging along with it some trees which had been planted round the spot. An old shaft, in front of Heaton Hall, has not, however, presented a like impediment, and consequently every exertion is using to open a communication by that way. They had uncovered the pit, and reached the scaffolding on Saturday the 6th, which was five fathoms from the surface; and we understand their efforts are likely to be successful, if not prevented by an accumulation of inflammable air, with which the old workings appear to be filled. Ever since the accident, three large engines (one of 130 horse power) have been constantly employed in drawing the water from the pit, at the rate of about 1200 gallons per minute, yet on Friday morning it was found to have attained the height of 81 fathoms up the shaft. In the evening, however, the water had decreased about three feet, and we understand has continued to decrease since that time; so that no doubt is now entertained of the colliery being at some future period again set to work. We now come to state the extent of the calamity. Mr. Miller (the underviewer, who has left a wife and eight children), 32 workmen, 42 boys, and 37 horses, have perished; and 25 widows, with about 80 children, are left to bemoan the sudden death of their husbands and fathers.

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* This account is partly drawn up from private information, and partly from the Tyne Mercury for May 9, 1815.

ARTICLE XIII.

New Patents.

JOHN CARPENTER, Truro; for a knapsack, which prevents the wet coming between it and the back; and a pouch in front suspended from the shoulder straps of said knapsack, so as to counteract its weight. Jan. 20, 1815.

JEAN RAUDONT, Oxford-street; for certain improvements in the construction of dioptric telescopes. Jan. 20, 1815.

JAMES MILLER, Liverpool; for certain improvements in the construction of stills, furnaces, chimneys, and other apparatus connected with the art of distillation. Jan. 28, 1815.

JOHN WOOD, Manchester, clock-maker; for certain improvements in machinery, used for preparing and spinning cotton-wool and various other articles. Feb. 4, 1815.

JOSEPH TAYLOR and PETER TAYLOR, Manchester, machine-makers; for certain improvements in the loom to be used in weaving cotton, worsted, silk, or other cloth, made of any two or more of the said materials. Feb. 4, 1815.

JAMES THOMSON, Primrose Hill, Lancashire; for certain improvements in the process of printing cloth made of cotton or linen, or both. Feb. 4, 1815.

WILLIAM GRIFFITH, Giltspur-street, London; for an improved toast-stand. Feb. 7, 1815.

RICHARD JONES TOMLINSON, Bristol; for certain improvements in the method of framing, constructing, or putting together the roofs of buildings, or the parts thereof. Feb. 9, 1815.

WILLIAM MOULT, Bedford-square, London; for a mode of evaporation and sublimation. Feb. 13, 1815.

JOSEPH BURRELL, Thetford, Norfolk; for an invention to be used as a support and safeguard in getting in and out of chaises, and other two-wheeled carriages. Feb. 21, 1815.

JONAH DYER, Wootton-under-edge, Gloucester; for an improved frame or machine for shearing of woollen cloth. Feb. 21, 1815.

SAMUEL BROWN, Mark Lane, London; for a rudder and certain apparatus connected therewith, for governing ships and vessels of all descriptions, with much more certainty and effect, and for producing various advantages not hitherto practised or known. Feb. 28, 1815.

RALPH DODD and GEORGE STEPHENSON, Killingworth, Northumberland; for various improvements in the construction of locomotive engines. Feb. 28, 1815.

WILLIAM MITCHELL, Glasgow, and JOHN LAWTON, King-street, Snow-hill, London; for a lock and key applicable to various purposes. March 7, 1815.

THOMAS DEAKIN, Ludgate-hill, London; for a portable kitchen. March 7, 1815.

DUDLEY ADAMS, Fleet-street, London; for certain improvements in the construction of paper vellum tubes for telescopes, and other optical parts of telescopes. March 7, 1815.

WILLIAM WOOD, Shadwell, Middlesex; for the manufacture of a material or materials, and the application thereof to the more effectually making water tight and sea-worthy ships and all other vessels, which he denominates adhesive felt. March 9, 1815.

ROBERT DICKINSON, Great Queen-street, Lincoln's Inn-fields, London; for certain improvements in the making or fabrication of sundry tools, implements, or articles, used in various arts or manipulations, or the ordinary occasions of life. March 14, 1815.

JOHN MILLS, Holywell-street, St. Clement's church-yard, Strand, London; for improved elastic stays for women and children; and also to give relief to women in a state of pregnancy. March 14, 1815.

ELIZABETH BEVERIDGE, Hatton-garden, London; for an improved bedstead. March 14, 1815.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1815.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
4th Mo.										
April 1	S E	29·72	29·55	29·635	70	45	57·5		3	☾
2	W	29·82	29·81	29·815	63	42	52·5			
3	Var.	29·91	29·81	29·860	57	34	45·5		3	
4	W	30·18	29·91	30·045	60	35	47·5			
5	W	30·20	30·12	30·160	59	41	50·0			
6	N W	30·12	30·06	30·090	66	43	54·5			
7	E	30·06	29·91	29·985	69	45	57·0			
8	E	29·91	29·81	29·860	61	45	53·0	·68		
9	N E	29·84	29·81	29·825	64	43	53·5		—	○
10	N E	29·90	29·84	29·870	54	50	52·0		·50	
11	Var.	29·90	29·89	29·895	63	47	55·0		—	
12	S W	29·89	29·75	29·820	68	40	54·0		4	
13	Var.	29·59	29·56	29·575	69	36	52·5		·23	
14	N W	30·00	29·59	29·795	46	34	40·0		—	
15	N	30·13	30·00	30·065	50	28	39·0	·30		
16	N E	30·13	30·11	30·120	53	30	41·5			☽
17	N E	30·23	30·11	30·180	54	31	42·5			
18	N	30·25	30·18	30·215	54	30	42·0		—	
19	Var.	30·18	29·85	30·015	59	38	48·5			
20	S W	29·85	29·17	29·510	59	36	47·5		—	
21	Var.	29·17	28·74	28·955	49	40	44·5		·72	
22	N	29·06	28·74	28·900	50	40	45·0	·38	·20	
23	N W	29·24	29·06	29·150	52	35	43·5		·16	●
24	N E	29·66	29·24	29·450	54	36	45·0		2	
25	Var.	29·91	29·66	29·785	53	41	47·0		8	
26	S E	30·06	30·04	30·050	58	33	45·5		—	
27	N E	30·04	29·92	29·980	61	43	52·0		1	
28	N E	29·92	29·64	29·780	64	45	54·5			
29	N E	29·64	29·49	29·565	54	39	46·5		—	
30	S E	29·65	29·47	29·560	56	40	48·0	·45	7	
		30·25	28·74	29·783	70	28	48·56	1·81	2·09	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fourth Month.—A summer-like day, with a breeze of wind: *Cirrus* formed about noon, and continued after sun-set, passing to *Cirrostratus*: some rain followed. 2. Overcast, a. m. apparently with *Cirrostratus* from *Cirrus*: during the middle of the day this gave place to *Cumulostratus*. 3. a. m. Overcast, calm, with dew on the grass: showers followed: after which *Cumulus* and *Cirrus*. 4. Much dew: *Cirrus*, followed by *Cirrostratus*: a few large drops about sun-set. 5. Much dew: *Cirrus*. 6. The same: low *Cumuli* prevailed afterwards, some of them capped with *Cirrostrati*: then a shallow bed of *Cumulostratus*, ending at sun-set in *Cirrostratus*: the evening twilight opake, dewy, and suffused with red. 8. *Cumulus*, beneath *Cirrocumulus*: a strong breeze: a mixture of *Cirrostratus* and *Cirrus* gave the clouds an appearance of active electricity; but the whole ended in *Cirrostratus*. 9. A confused mixture of the modifications, as yesterday, with the addition of haze: some drops by 10 a. m. and a shower, p. m. 10. Heavy showers, a. m.: cloudy, p. m. 11. Wind, a. m. S. W.: the clouds, mingled with the smoke of the city, came back from the E. at sun-set. 12. a. m. The clouds gathered pêle mêle, the *Cumulus* capping: p. m. the *Nimbus* appeared, and after some distant thunder to S. W. we had a shower. 13. a. m. *Cirrus*, *Cirrocumulus*: much dew, the large drops of which sparkled in the sun with the prismatic colours: various modifications of cloud followed: about one p. m. it thundered, N.: then nearer, with lightning, S.: the wind shifting to that point, we had a heavy shower from W. with hail: wind and rain at night. 14. a. m. Cloudy, windy: *Cumulostratus*: a little snow: swallows skimming the meadows. 15. *Cumulus*, succeeded by *Cumulostratus*: the wind moderate. 16. The same: there were indications of hoar frost this morning. 17. Hoar frost: *Cumulostratus*: at night *Cirrostratus*, and a lunar corona. 18. a. m. A few drops during the union of some clouds, which became *Cumulostratus*. 19. a. m. Loose *Cumulus*: at night *Cirrus*, and afterwards a very large *white* lunar halo. 20. Overcast, dripping: a hail shower, p. m.: at night a lunar halo, very small, the prismatic ring touching a corona within. 21. Much cloud and wind: wet forenoon and night: the greatest depression of the barometer that has occurred in the present year. 22, 23. Much cloud, windy: rain at intervals. 24. Windy: cloudy: rain, a. m. with a little hail: in the evening the *Cumuli* dispersed rapidly, with *Nimbi* and *Cirrostrati* in the horizon. 25. Various clouds, ending in rain, of which, however, very little till night. 26, 27. Windy, variable: some dripping. 28. A little rain early: then *Cumulostratus*. 29. *Cumulostratus*: strong breeze: dripping: the *Cirrocumulus* appeared, transiently, as for several days past: a *Stratus* at night. 30. *Cirrocumulus*, which passed to *Cirrostratus*: p. m. (upon the wind becoming S. E.) rain.

RESULTS.

Winds variable, but for the most part Northerly.

Barometer:	Greatest height	30·25 inches.
	Least	28·74
	Mean of the period	29·783
Thermometer:	Greatest height	70°
	Least	28
	Mean of the period	48·56

Evaporation, 1·89 inch. Rain, 2·09 inches.

Tottenham, *Fifth Month*, 2, 1815. L. HOWARD.

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